



AGRICULTURAL RESEARCH INSTITUTE
PUSA

PHILOSOPHICAL
TRANSACTIONS,
GIVING SOME
ACCOUNT
OF THE
Present Undertakings, Studies, *and* Labours,
OF THE
INGENIOUS,
IN MANY
Considerable Parts of the WORLD.

VOL. LIX. For the Year 1769.

L O N D O N :

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M.DCC.LXX.

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations, which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume. And this information was thought the more necessary, not only as it has been the common opinion, that they were published by the authority, and under the direction, of the Society itself; but also, because several authors, both at home and abroad, have in their writings called them the *Transactions of the Royal Society*. Whereas in truth the Society, as a body, never did interest themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable, that a Committee of their Members should be appointed to reconsider the papers read before them, and select out of them such, as they

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should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance or singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the chair, to be given to the authors of such papers, as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports, and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

C O N-

C O N T E N T S

T O

V O L. LIX.

- I. *A Letter from Mr. J. Moulton to Dr. Percival, of Manchester, F. R. S. containing a new Manner of preparing Saltp.* p. 1
- II. *Brevis Narratio de Structura et Effectu Speculorum causticorum parabolicorum à defuncto D^{no} Hoefen Dresdae elaboratorum, quae nunc à D^{no} Ehrhard, sub Arce Dresdenfi habitante, possidentur. Auctore D^{no} Wolfe, M. D.* p. 4
- III. *An extraordinary Case of three Pins swallowed by a Girl, and discharged at her shoulder. In a Letter to Frank Nicholls, M. D. F. R. S. from Dr. Lysons, of Gloucester.* p. 9
- IV. *A Letter from the Honourable William Hamilton, his Majesty's Envoy Extraordinary at Naples, to Mathew Maty, M. D. Sec. R. S. containing some farther Particulars on Mount Vesuvius, and other Volcanos in the Neighbourhood.* p. 18
- V. *A*

- V. *A Letter to Dr. William Watſon, F. R. S. from the Honourable Daines Barrington, F. R. S. on the Trees which are ſuppoſed to be indigenous in Great Britain.* p. 23
- VI. *An Account of a Caſe in which the upper Head of the Os Humeri was ſawed off, a large Portion of the Bone afterwards exfoliated, and yet the entire Motion of the Limb was preſerved. By Mr. White, Surgeon, at Manchester. Communicated by Mr. Watſon, F. R. S.* p. 39
- VII. *Letters from the Reverend Dr. William Borlaſe, F. R. S. Rector of Ludgvan in Cornwall, to Charles Morton, M. D. F. R. S. and from Mr. Roſewarne, of Truro, to Dr. Borlaſe; giving an Account of a Specimen of Native Tin found in Cornwall, and now depoſited in the Muſeum of the Royal Society.* p. 47
- VIII. *An Account of an Eſſay on the Origin of a natural Paper, found near the City of Cortona in Tuſcany. In a Letter from John Strange, Eſq; F. R. S. to Mathew Maty, M. D. Sec. R. S.* p. 50
- IX. *Experiments on the lateral Force of Electrical Ex- ploſions. By Joſeph Prieſtley, LL. D. F. R. S.* p. 57
- X. *Various Experiments on the Force of Electrical Ex- ploſions. By Joſeph Prieſtley, LL. D. F. R. S.* p. 63
- XI. *Abſtract of a Letter from Stephen De Viſme, Eſquire, at Canton, in China, to Henry Baker, Eſquire, F. R. S. containing an Account of an Earth- quake at Macao, and a ſhort Deſcription of a ſingular Species of Monkeys without Tails, found*
in

C O N T E N T S.

vii

in the interior Part of Bengal. Communicated by Mr. Baker. p. 71

XII. *A Letter from Mr. John Robertson, Lib. R. S. to James West, Esq; President of the Royal Society; containing the Demonstration of a Law of Motion, in the Case of a Body deflected by two Forces tending constantly to two fixed Points.* p. 74

XIII. *A Letter from the Reverend Mr. William Paxton, Rector of Buckland Brewer, in the County of Devon, to Dr. Milles, Dean of Exeter, F. R. S. and Pr. S. A.* p. 79

XIV. *Abstract from a Meteorological Register kept at the Royal Hospital near Plymouth, during the Year 1768. By W. Farr, M. D. Communicated by W. Watson, M. D. F. R. S.* p. 81

XV. *An Account of a remarkable Aurora Borealis, observed at the Observatory of the Marine at Paris, by M. Meslier, of the Royal Academy of Sciences, and F. R. S. Translated by J. Bevis, M. D. F. R. S.* p. 86

XVI. *Observations on the Expectations of Lives, the Increase of Mankind, the Influence of great Towns on Population, and particularly the State of London with respect to Healthfulness and Number of Inhabitants. In a Letter from Mr. Richard Price, F. R. S. to Benjamin Franklin, Esquire, LL.D. and F. R. S.* p. 89

XVII. *Dissertatio epistolaris de Ossibus et Dentibus Elephantum, aliarumque Belluarum in America Septentrionali, aliisque borealibus Regionibus observatis; qua indigenarum Belluarum esse ostenditur. Auctore R. E. Raspe, Serenissimo Hassiarum Landgravo à Consiliis, et R. S. S.* p. 126

XVIII. *Obser-*

- XVIII. *Observations on a particular Manner of Encrease in the Animalcula of vegetable Infusions, with the Discovery of an indissoluble Salt arising from Hemp-seed put into Water till it becomes putrid.*
By John Ellis, Esquire, F. R. S. p. 138
- XIX. *On the Computation of the Sun's Distance from the Earth, by the Theory of Gravity: In a Letter to Mathew Maty, M. D. Sec. R. S. from the Rev. Mr. Horsley, F. R. S.* p. 153
- XX. *Meteorological Observations for 1768, made at Bridgwater in Somersetshire, and at Ludgvan in Mount's-Bay, Cornwall. Communicated by Dr. Jeremiah Milles, Dean of Exeter, and F. R. S.* p. 155
- XXI. *Proposal of a Method for securing the Cathedral of St. Paul's from Damage by Lightning; in consequence of a Letter from the Dean and Chapter of St. Paul's to James West, Esquire, Pr. R. S.* p. 160
- XXII. *Observation of the late Transit of Venus; In a Letter to James Burrow, Esquire, V. P. R. S. By Mr. James Horsfall, F. R. S.* p. 170
- XXIII. *An Account of the Observations of the Transit of Venus and of the Eclipse of the Sun, made at Shirburn Castle and at Oxford. By the Reverend Thomas Hornsby, M. A. F. R. S. and Savilian Professor of Astronomy in the University of Oxford.* p. 172
- XXIV. *Venus observed upon the Sun at Oxford, June 3, 1769: By Samuel Horsley, LL. B. Rector of St. Mary, Newington, in Surrey, F. R. S.* p. 183
- XXV. *Observations of the last Transit of Venus, and of the Eclipse of the Sun the next Day; made*

CONTENTS.

ix

- made at the House of Joshua Kirby, Esquire, at Kew. By John Bevis, M. D. F. R. S.* p. 189
- XXVI. *A Letter to the Astronomer Royal from John Canton, M. A. F. R. S. containing his Observations of the Transit of Venus, June 3, 1769, and of the Eclipse of the Sun the next Morning.* p. 192
- XXVII. *An Account of several sepulchral Inscriptions and Figures in Bas-relief, discovered, in 1755, at Bonn, in Lower Germany. In a Letter to James West, Esquire, Pr. R. S. from John Strange, Esquire, F. R. S.* p. 195
- XXVIII. *An Account of the Lymphatic System in Amphibious Animals. By Mr. William Hewson, Lecturer in Anatomy: In a Letter to William Hunter, M. D. F. R. S. and by him communicated to the Society.* p. 198
- XXIX. *An Account of the Lymphatic System in Fish. By the same.* p. 204
- XXX. *A Letter from Mr. Lane, Apothecary, in Aldersgate-street, to the Honourable Henry Cavendish, F. R. S. on the Solubility of Iron in simple Water, by the Intervention of fixed Air.* p. 216
- XXXI. *Account of several Phaenomena observed during the Ingress of Venus into the Solar Disc. By the Reverend W. Hirst, F. R. S. in a Letter to the Astronomer Royal.* p. 228
- XXXII. *Observations made at Leicester on the Transit of Venus over the Sun, June 3, 1769. By the Reverend Mr. Ludlam, Vicar of Norton, near Leicester.* p. 236
- VOL. LIX. b XXXIII. A

- XXXIII. *A Letter from John Hope, M. D. F. R. S. Professor of Physic and Botany in the University of Edinburgh, to William Watson, M. D. F. R. S. on a rare Plant found in the Isle of Skye.* p. 241
- XXXIV. *Astronomical Observations made by Samuel Holland, Esquire, Surveyor-General of Lands for the Northern District of North-America; and others of his Party. Communicated by the Astronomer Royal.* p. 247
- XXXV. *Observations made on the Island of Hammerfest, for the Royal Society. By Jeremiah Dixon.* p. 253
- XXXVI. *Astronomical Observations made at the North Cape, for the Royal Society. By Mr. Bayley.* p. 262
- XXXVII. *An Account of an Observation of the Transit of Venus, made at the Isle Coudre near Quebec. In a Letter to the Reverend Nevil Maskelyne, Astronomer Royal, from Mr. Thomas Wright, Deputy Surveyor of the Northern District of America.* p. 273
- XXXVIII. *Extract of a Letter from Mr. B. Gooch, Surgeon, of Shottisham, near Norwich, to Mr. Joseph Warner, F. R. S. and Surgeon to Guy's Hospital. Communicated to the Royal Society by Mr. Warner.* p. 281
- XXXIX. *Observatio ingressus Veneris in Solem, 3 die Junii, 1769, habita Gryphiswaldiæ, ab Andr. Mayer, Prof. Reg. Communicated by Mr. John Ellicott, F. R. S.* p. 284
- XL. Obser-

XL. *Observation of a Solar Eclipse the 4th of June, 1769, at the Observatory at Aulthorpe, near Leeds, in the County of York. By J. Smeaton, F.R.S.*

p. 286

XLI. *Account of the Transit of Venus over the Sun's Disc, as observed at Norriton, in the County of Philadelphia, and Province of Pennsylvania, June 3, 1769. By William Smith, D.D. Provost of the College of Philadelphia; John Lukens, Esquire, Surveyor General of Pennsylvania; David Rittenhouse, A.M. of Norriton; and John Sellers, Esquire, one of the Representatives in Assembly for Chester County; the Committee appointed for that Observation, by the American Philotophical Society, held at Philadelphia, for promoting useful Knowledge. Communicated to the said Society, in Behalf, and by Direction, of the Committee, by Doctor Smith; and to the Royal Society of London, by Nevil Maskelyne, B.D. Astronomer Royal.*

p. 289

XLII. *Observationes Transitus Veneris per Discum Solis, die 3 Junii, 1769, habitæ in Succia, et Societati Regiæ Londinenfi communicatæ à Petro Wargentini, ejusdem Societatis Sodali, et Academiæ Regiæ Scient. Stockholmenfis Secretario. Communicated by Mr. John Ellicott, F.R.S.*

p. 327

XLIII. *Observations of the Transit of Venus over the Sun, contained in a Letter to the Reverend Nevil Maskelyne, Astronomer Royal, from Dr. Alexander Willon, Professor of Astronomy in the University of Glasgow.*

p. 333

- XLIV. *An Account of the late Transit of Venus, observed at Hawkhill, near Edinburgh. In a Letter to the Astronomer Royal, from James Lind, M. D. at Edinburgh. To which are added, some Remarks by the Astronomer Royal; and further Particulars relative to the Observations communicated in other Letters.* P. 339
- XLV. *Observations of the Transit of Venus, and other Astronomical Observations, made at Gibraltar; contained in a Letter to the Astronomer Royal from Lieutenant Jardine.* P. 347
- XLVI. *Observations of the Transit of Venus over the Sun, June 3, 1769. In a Letter to the Reverend Nevil Maskelyne, F. R. S. Astronomer Royal, from John Winthrop, Esquire, F. R. S. Hollisian Professor of Mathematics at Cambridge, in New-England.* P. 351
- XLVII. *Of the different Quantities of Rain, which appear to fall, at different Heights, over the same Spot of Ground. By William Heberden, M. D. F. R. S.* P. 359
- XLVIII. *An Account of an Observation of an Eclipse of the Moon, made at Hawkhill, near Edinburgh. In a Letter to the Astronomer Royal, from James Lind, M. D.* P. 363
- XLIX. *An Account of Two Auroræ Boreales observed at Oxford. In Two Letters to Mathew Maty, M. D. Sec. R. S. from the Reverend John Swinton, B. D. F. R. S. Custos Archivorum of the University of Oxford, Member of the Academy degli Apatisti at Florence, and of the Etruscan Academy of Cortona in Tuscany.* P. 367
L. *Obser-*

- L. *Observations of the Transit of Venus on June 3, 1769, and the Eclipse of the Sun on the following Day, made at Paris, and other Places. Extracted from Letters addressed from M. De la Lande, of the Royal Academy of Sciences at Paris, and F. R. S. to the Astronomer Royal; and from a Letter addressed from M. Messier to Mr. Magalhaens.* p. 374
- LI. *Transit of Venus over the Sun, observed June 3, 1769, by Alexander Aubert, in Austin Friars, London, three Seconds of Time East of St. Paul's, with a Cassegrain Reflector of J. Short, having a Metal Speculum of two Feet focal Length, and magnifying about 110 Times.* p. 378
- LII. *Some Account of an Oil, transmitted by Mr. George Brownrigg, of North Carolina. By William Watson, M. D. R. S. S.* p. 379
- LIII. *A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society by the worshipsful Company of Apothecaries, for the Year 1767, pursuant to the Direction of Sir Hans Sloane, Bart. Med. Reg. et Soc. Reg. nuper Præsēs: By William Hudson, Societatis Regiæ & clariss. Societatis Pharmaceut. Lond. Soc. Hort. Chelsean. Præfectus et Prælector Botanicus.* p. 384
- LIV. *A Description of the Lymphatics of the Uretbra and Neck of the Bladder. By Henry Watson, Surgeon to the Westminster Hospital, and F. R. S.* p. 392
- LV. *Eclipses of Jupiter's First Satellite, the Eclipse of the Moon, and Occultations of Fixed Stars by the Moon. Observed at the Royal Observatory at Greenwich, in the Year 1769. Communicated by the Astronomer Royal.* p. 399
- LVI. *Eclipses*

- LVI. *Eclipses of Jupiter's First Satellite, with an Eighteen Inch Reflector of Mr. Short's. Observed by Dr. Wilson at the Glasgow Observatory.* p. 402
- LVII. *Extract of a Letter to the Reverend Nevil Maskelyne, Astronomer Royal, from Mr. Benedict Ferner, F.R.S. Dated Stockholm, June 9, 1769. Translated from the French.* p. 404
- LVIII. *Observations of the Transit of Venus over the Sun, on June 3, 1769, and the Eclipse of the Sun the next Morning; made at East Dereham in Norfolk, by the Reverend Francis Wollaston, F. R. S. Extracted from some Letters addressed to the Reverend Nevil Maskelyne, F. R. S. and Astronomer Royal.* p. 407
- LIX. *Observations of the Transit of Venus over the Sun, June 3, 1769; made by Mr. Owen Biddle and Mr. Joel Bayley, at Lewestown, in Pennsylvania. Communicated by Benjamin Franklin, LL.D. F. R. S.* p. 414
- LX. *Observations of the Transit of Venus over the Sun, made at the Round Tower in Windsor Castle, June 3, 1769. By Daniel Harris, Master of the Royal Mathematical School in Christ's Hospital, and F. R. S. In a Letter to the Reverend Nevil Maskelyne, B. D. F. R. S. and Astronomer Royal.* p. 422
- LXI. *An Attempt to elucidate two Samnite Coins, never before fully explained. In a Letter to Mathew Maty, M. D. Sec. R. S. from the Reverend John Swinton, B. D. F. R. S. Custos Archivorum of the University of Oxford, Member of the Academy degli Apatisti at Florence, and of the Etruscan Academy of Cortona in Tuscany.* p. 432
- LXII. Ob-

- LXII. *Observation of the Transit of Venus, on June 3, 1769. In a Letter from John Leeds, Esquire, Surveyor General of the Province of Maryland, to John Bevis, M. D. F. R. S.* p. 444
- LXIII. *Experiments to prove that the Luminousness of the Sea arises from the Putrefaction of its Animal Substances. By John Canton, M. A. and F. R. S.* p. 446
- LXIV. *A Series of Astronomical Observations made at the Observatory of the Marine at Paris, to wit, 1°. Observations of Jupiter's Satellites in the Years 1767 and 1768. 2°. Observations on the Shadows of Jupiter's Satellites. 3°. On the Variation of the Belts on the Disc of that Planet. 4°. Observation of a Spot on the Disc of the Third Satellite. 5°. Observation of the Belts of Saturn. 6°. Observation of the Moon's Passage over the Pleiades, in 1767. 7°. Observation of a partial Eclipse of the Moon, January 3, and of a total one, December 23, 1768. 8°. Observations of Two Auroræ Boreales, August 6, and December 5, of the same Year. By M. Messier, Astronomer of the Marine, F. R. S. and of the Academies of Holland and Italy.* p. 454
- LXV. *Astronomical Observations made by Order of the Royal Society, at Prince of Wales's Fort, on the North-west Coast of Hudson's Bay. By William Wales and Joseph Dymond.* p. 467
- LXVI. *Extract from the Journals of the Royal Society, June 23, 1768, respecting a Letter addressed to the Society by a Member of the House of Jesuits at Pekin in China; by Charles Morton, M. D.* See.

Sec. R. S. and Fellow of the Imperial Acad. Natur. Curios. et Petropol. and of the Royal Academy of Gottingen. p. 489

LXVII. *Observations of the Transit of Mercury, October 25, 1743. In a Letter to Professor Bliss, late Astronomer Royal, and F. R. S. from John Winthrop, F. R. S. Professor of Astronomy in Cambridge, New England.* p. 505

LXVIII. *A Method of working the Object Glasses of Refracting Telescopes truly spherical. By the late Mr. James Short, F. R. S.* p. 507

P R E S E N T S

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ROYAL SOCIETY

In the YEAR 1769;

W I T H

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PHILOSOPHICAL
TRANSACTIONS.

- I. *A Letter from Mr. J. Moulton to Dr. Percival, of Manchester, F. R. S. containing a new Manner of preparing Salep.*

S I R,

Read Jan. 12,
1769.

AS the specimen of Salep, which I left you some time ago, meets with your approbation, so far as to think it deserving to be laid before the Royal Society, I now send you my method of curing the common Orchis roots of our own country, so as perfectly to resemble what comes to us from Turkey. And if the communication be of any public utility, I shall think myself sufficiently gratified for the trouble I have had in prosecuting the experiments necessary thereto.

VOL. LIX.

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The

The roots I have hitherto made use of, are those of the *orchis morio mas foliis maculatis* of Parkinson, the *cynorchis morio mas* of Gerard, and the *cynorchis major*, *vulgo* dog-stones: though, from a specimen of the *orchis palmata major mas* of Gerard, which you have among the Salep, that root likewise appears capable of being made to answer the same purposes as the others. The best time to gather the roots is, when the seed is formed, and the stalk going to fall; for then the new bulb, of which the Salep is made, is arrived to its full size, and may be known from the old one, whose strength is then spent by the preceding germination, by a white bud rising from the top of it, which is the germ of the plant of the succeeding year. This new root, being separated from the stalk, is to be washed in water, and a fine thin skin, that covers it, to be taken off with a small brush; or, by dipping in hot water, it will come off with a coarse linen cloth.

When a sufficient quantity of the roots is thus cleaned, they are to be spread on a tin plate, and set into an oven, heated to the degree of a bread-oven, where they are to remain six, eight, or ten minutes; in which time they will have lost their milky whiteness, and have acquired a transparency like that of horn, but without being diminished in size. When they are arrived at this state, they may be removed to another room to dry and harden, which will be done in a few days; or they may be finished in a very slow heat, in a few hours. I have tried both ways with success.

The orchis's above-mentioned grow spontaneously in this part of the country, and throughout the whole kingdom.

kingdom. They flourish best in a dry, sandy, barren soil. As the method of curing this root is so easy, I hope it will encourage the cultivation of so nutritious a vegetable, so as to reduce it from its present high price, which confines it to people of fortune, to one so moderate as would bring it into common use, like other kinds of meal or flour; and so become a valuable addition to our present list of eatables, its quality of thickening water being to that of fine flour nearly as $2\frac{1}{2}$ to 1, with this difference, that the jelly of Salep-powder is clear and transparent, whereas that of flour is turbid and white.

If this should find you in the same sentiments respecting it, I give you liberty to make use of it accordingly.

And am,

with all respect,

Your very humble servant,

Rochdale, Nov.
10, 1768.

J. Moulst.

Received November 9, 1769.

II. *Brevis Narratio de Structura et Effectu Speculorum causticorum parabolicorum à defuncto D^{no} Hoefen Dresdae elaboratorum, quae nunc à D^{no} Ehrard, sub Arce Dresdenfi habitante, possidentur. Auctore D^{no} Wolfe, M. D.*

Read Jan.
12, 1769.

ORBIS vel rectius segmentum parabolicum concavum ex pluribus assensibus solidioris ligni compositum, et in convexa parte baculis tam a vertice divergentibus, quam transversis probe colligatum et firmatum, obductum est in concava parte laminis aurichalceis, quarum crassities $\frac{1}{4}$ circiter pollicis, longitudo pedum quatuor cum dimidio, latitudo vero pedum duorum cum dimidio. Hae laminae tanto studio inter se sunt conferruminatae, ut linea jungens duas tales laminas vix appareat: splendore vero tali pollent, qualis summa cum cura poliendo conciliari aurichalco potest. Suspenditur et movetur tale speculum inter duo brachia semicirculi lignei, mobilis circa axim ligneum verticalem, insistentem pedibus tribus, eorumque rotulis. Pondus pro ratione magnitudinis valde exiguum, ut unica manu in omnem situm facile dirigi possit. Anteriorem

riorem speculi partem subtendit arcus ferreus $\frac{1}{2}$ pollicem crassius, versus medium, hoc est in ipso loco foci ustorii, in anulum efformatus, qui annulus ex utraque parte furcam ferream gerit, quibus vasculum ferreum corpus examinandum continens imponitur. Possidet D^{nus} Erhard sex talia specula, quorum quatuor magis perfecta sequentes habent dimensiones.

Num.	Perimeter.		Diameter seu Ordinata.		Profunditas seu Abscissa.		Distantia foci a vertice.	
	Ped.	Poll.	Ped.	Poll.	Ped.	Poll.	Ped.	Poll.
1.	29	4	9	7	1	4	4	0
2.	21	0	6	8	0	$10\frac{1}{4}$	3	1
3.	16	4	5	1	0	$10\frac{1}{2}$	1	10
4.	13	$2\frac{1}{2}$	4	2	0	7	1	9

Pes Dresdensis ad Londinensem fere ut 13:14. Primum speculum pro faciliori transportatione in duas partes divisum est. Curvaturam horum speculorum satis exactam esse, patet ex eo, quod diameter foci priorum quatuor dimidium pollicem non excedat. Utinam color et politura speculorum metallicorum iis conciliari posset! Effectus horum, urendo, calcinando, fundendo, vitrificando, multum superat quodcumque de hujus generis experimentis innotuit. Durissimi lapides vix pauca secunda temporis resistunt: metalla cito perforantur: vegetabilia nictu oculi comburuntur in cinerem et vitrum, aque ac ossa animalium in calcem et vitrum.

Lubet hic referre pauca experimenta a D^{no} D^o Hoffman, nuper instituta, cum speculo quod tertium in ordine.

1. Minera argenti nativi in lapide talcofo viridi, fundebatur tempore unius secundi, absque ullo fumo.

Post unius minuti primi fusionem inveniebantur per multa argenti granula lapidi in vitrum nigrum mutato adhærentia.

2. Minera cupri pyriticosa Salsendensis cum malachite intersperso fluebat, uti præcedens, in momento : et tribus secundis vix elapsis, guttæ rubræ depluebant, quæ frigefactæ cuprum fragile erant ; lapis vero nunc erat vitrum ex rubro nigrum.

3. Minera stanni polyedra Altenburgensis in momento fluens, post unius minuti fusionem, inveniebatur multa stanni grana dimisisse ; ipse vero lapis quarzofus in vitrum nigrum mutatus erat.

4. Galena plumbi tessulis majoribus in nictu oculi fundebatur cum fumo denso, et post tria secunda temporis plumbum bonum malleabile depluebat.

5. Hæmatites niger striatus quarto temporis secundo incipiebat fundi, sine omni fumo, et post duo minuta prima ablato, perfecti ferri aliquid adhærere cernebatur.

6. Asbestus Hungaricus tempore trium secundorum in vitrum abibat ex flavo viride.

7. Glacies Mariæ Eislebensis in momento calcinabatur ; post 4 vel 5 minuta prima in loco foci perforabatur, atque in vitrum ex viridi flavum vertebatur, simile illi quod ex asbesto provenit.

8. Schistus vulgaris niger tegularis tempore duorum secundorum abibat in vitrum nigrum.

9. Marmor nigrum in loco, quem focus attingebat, tempore aliquot secundorum calcinabatur, et post unum minutum primum hinc inde fundi incipiebat.

10. Frustum lapidis sabulosi martialis tempore trium secundorum in vitrum nigrum fundebatur.

11. Num-

11. Nummus aureus Hispanicus (*a pistole*) fundebatur intra duo secunda, fusus niger apparebat.

12. Florenus Saxonicus (*half a crown*) in momento fundi incipiebat, et intra tria secunda jam perforatus erat.

13. Clavus ferreus ex rota, tempore trium secundorum fluebat, et tempore quinque jam tres ferri guttæ pisi majoris magnitudine confluerant in unam, insimulque vitri viridis parum productum erat.

14. Si ignis cum flamma intra focum et speculum ponatur, tempore nocturno obscuro, magna circumjacens plaga ita illuminatur, ut etiam in horologio turris distantis numerus horam indicans discernatur.

15. Si ignis prunarum bene accensarum ponatur directe ante speculum, licet notabili distantia, calor in foco satis intensus percipitur, ut etiam candela accendi, vel aliæ res inflammabiles comburi possint.

16. Si vero prunæ accensæ in ipso foco ponantur, et radii inde egressi a speculo reflexi ab alio speculo in distantia satis notabili excipiantur, poterunt res inflammabiles in hujus secundi speculi foco positæ accendi.

17. Experimenta duo ultima non solum succedunt cum ipsis prunis accensis, sed etiam, cum fornace fortiter calefacto, in foco speculi posito, vel juxta experimentum 15^{um} ante eundem, distantis nempe probe selectis.

18. In camera obscura imagines objectorum ab his speculis reflexæ distincte et cum propriis coloribus repræsentantur. Præterea cum microscopio solari hæc specula utiliter combinari possunt.

19. Si duo specula in notabili distantia, uti 50 passuum, directe sibi invicem opponantur, et in
foco

foco unius verba quædam voce admodum depressa proferantur, vel horologium minus marsupiale ibi ponatur, poterunt ista verba vel horologii vibrationes in foco alterius speculi distincte audiri.

Qu. Specula Archimedeæ erantne diversa ab his? Certe parabola, cujus parameter bis mille pedum, non difficulter describitur. Forte etiam radii a tali speculo reflexi, post focum a lente excipi, et situ parallelo ad omnem distantiam mitti possent, habita nempe ratione fusibilitatis vitri.

Sept. 2, 1768.

Received December 3, 1768.

III. *An extraordinary Case of three Pins swallowed by a Girl, and discharged at her Shoulder. In a Letter to Frank Nicholls, M. D. F. R. S. from Dr. Lysons, of Gloucester.*

To Charles Morton, M. D. Sec. R. S.

Epſom, Nov. 25, 1768.

DEAR SIR,

Read January 26,
1769.

INCLOSED I ſend you a moſt extraordinary caſe, which is tranſmitted to me by Dr. Lyſons, a gentleman of great learning and credit, and phyſician to the Glouceſter hoſpital. It ſeems to be exactly drawn, and the Doctor's veracity may be depended on. I think it well worth preſerving in the Memoirs of the Society; and believe that the Council will have the ſame opinion of it as,

SIR,

Your very humble ſervant,

Fran. Nicholls.

S I R,

UPON my mentioning the case of a girl who swallowed three pins, which were afterwards discharged at her shoulder, you thought it might be proper for the Philosophical Transactions, and desired me to send it you. I have drawn it from notes taken during my attendance upon her, with as much accuracy as possible, and it is as follows.

Eleanor Kaylock, a robust, strong girl, aged twenty-two, was admitted a patient in the Gloucester infirmary, May 29, 1766, for a pain in her side proceeding from pins swallowed three quarters of a year before. The occasion of the accident was thus. Being employed in the business of a kitchen, as she was scumming the pot (her mouth being open, and three pins in it), she received a quantity of the vapour, which obliged her to swallow, and the pins at the same time passed into the *œsophagus*, where they remained for eight weeks, notwithstanding various methods were used for their removal; but they were at last forced down by the whalebone instrument used by surgeons for that purpose.

Whilst the pins were in her throat, the parts became inflamed, and swollen, which occasioned an hoarseness, attended with great pain, and difficulty of breathing: being also capable of receiving but very little nourishment, and that liquids, she was reduced to so weak a state as not to be able to get out of her bed. After the pins were removed she could swallow solids, and recovered strength sufficient to go out again to service in her former employment. She was hired as an under-servant in a gentleman's kitchen,

chen, but was soon obliged to quit her place, and apply for relief, any extraordinary motion aggravating her complaints, and occasioning violent convulsions, from which she did not recover for eight or nine hours. When she came to the infirmary, she appeared full of flesh, of a ruddy complexion, and in perfect health, excepting the following complaints.

She had a pain in her right side, below the false ribs, which she first felt immediately upon the removal of the pins from the *æsophagus*, and it continued to the time of her admission at the hospital, but was most violent when she moved the trunk of her body forwards round towards the left, or lifted up her right arm. At her admission, and from the time of the removal of the pins, the hoarseness she was troubled with soon after the pins first stuck in her throat, continued; she often spit up blood, and had a violent cough, by which, as well as by labour, or any excess of motion, the pain in her side being greatly aggravated, she was obliged to sit or fall down immediately, and could not recover herself, so as to be able to stand, in less than an hour. In these paroxysms she had always a pain in her head, was sick at stomach, and frequently brought up blood.

Whilst she was in the infirmary, the violence of the pain three times occasioned convulsion-fits, by which the *musculus rectus superior* of the right eye was so violently affected, that, notwithstanding the eye was open, yet the pupil was entirely covered by the eye-lid; and, after one of these fits, continued so for a fortnight. The left eye was also inverted in the

same manner, but the constriction was removed in a week. When these spasmodic affections left her, she did not recover her eye-sight for some days, the optic nerve being probably oppressed; but the left eye always recovered sooner than the right, being never so strongly convulsed. None of the other muscles appeared to be affected, except in the paroxysms.

While the pins were in the *asphagus*, the surgeon was utterly at a loss where to direct his instruments, as there was no certain indication where the pins were lodged. And the physician's practice could be only palliative, using bleeding, with anodyne and lubricating medicines, according as the various symptoms occasionally required. In this manner things went on to the beginning of August, when a small painful tumour, the size of a man's thumb, appeared upon the right shoulder, which disappeared in the compass of a week without coming to suppuration. Afterwards such another small tumour appeared upon the left shoulder, which increased, and, by the care of Mr. Crump, the attending surgeon, was brought to suppuration, and opened by him, August 20, when a large table spoonful of matter was discharged. Upon removing the dressings, the next day, a larger quantity of matter flowed out, and with it issued one of the pins. Mr. Crump then examined with his probe if he could find either of the others, but could not: however, the day following, the other two pins were also discharged at the same wound. These pins were all of the same length, each measuring five quarters of an inch. The wound at which these pins were discharged was upon the superior part of the *scapula*. After the girl had received her cure, and
was

was discharged from the infirmary (which happened September the fourth), I compared her shoulder with Cowper's Anatomical Tables on the Muscles; and, as near as I can guess, the wound was upon the fleshy belly of the *trapezius*. And yet the pain in the patient's side attended her as long as the pins remained in the wound, but left her soon after they were discharged, as did also her cough, and spitting of blood. Being obliged to lead a sedentary life in the infirmary, and to keep herself as quiet as possible, her *catamenia* left her; but her spitting of blood could not be attributed to that defect, because she was very regular before her admission, and yet she had spit blood from the time the pins were removed from the *œsophagus*, which was some months before she came to the infirmary.

It would be matter of considerable satisfaction, could the exact course be ascertained which was taken by these pins, in their passage from the *œsophagus* to their exit at the left shoulder. From the cough and spitting of blood one should suppose that the lungs were injured by them. From the pain under the false ribs, it may be imagined that the diaphragm was affected. And yet from their being discharged at the shoulder it may be presumed, that neither of these parts were ever wounded; but that the pins, being forced through the substance of the *œsophagus* into the muscles of the neck and shoulder, passed thence to the part whence they were discharged.

The first symptom observable upon the removal of these pins from the passage of the *œsophagus* was, that the patient immediately felt a pain in her right
side,

side, below the false ribs, which was most violent when she turned the trunk of her body forwards round towards the left, or lifted up her right arm. Now if the pins, being forced out of the *asphagnus*, penetrated the *serrati*, *rhomboides*, and *trapezius* muscles on the right side, this symptom must necessarily happen. For the *serrati* being muscles of respiration, and the *serratus superior posticus* attached to the second, third, fourth, fifth, and sixth ribs; and the *serratus inferior posticus* being attached to the tenth, eleventh, and the extremity of the twelfth ribs, a pain in the side will be produced by the constant efforts of respiration. And the office of these muscles being to elevate the ribs, and draw down the arm, the pain in the side will be most sensibly felt whenever the right arm is lifted up; because then the extremities of these muscles, attached to the ribs, will be most tense. For although a wound may be given to a muscle in its most fleshy part, yet the irritation occasioned by it will exert itself most forcibly in that part where there is the greatest tension.

The *rhomboides* muscle lying upon the *serratus superior*, and the *trapezius* being incumbent upon it, and all closely connected by the cellular membrane, they must all be in some degree affected by respiration. But the office of the *rhomboides* and *trapezius* muscles being to draw the arm downwards, and backwards, the pain in the side would be increased whenever the right arm and trunk of the body were turned forwards towards the left side.

Being thus, as we may suppose, arrived at the true cause of the pain in the side, the cough comes next under consideration. And this will be found to proceed

ceed from the same cause that the cough of a pleuritic person does, only with this difference, that in one the *pleura* and intercostal muscles are affected by an internal inflammation, by which respiration is disturbed; in the other, the malady arises from irritation caused by an extraneous body. The effects are the same in both; respiration being impeded, nature endeavours to relieve herself by a cough, which increases the irritation and inflammation of the parts obstructed; these again increase the violence of the cough: and thus, each being aggravated by the other, the lungs are often so violently agitated, that a blood-vessel bursts, and thence blood is thrown up from the lungs, as was the case in the present instance.

Whoever considers the communication between the third pair of nerves, the intercostal, the cardiac, and the recurrenents, together with the other nerves dependent upon them, will easily perceive the cause of the violent spasm upon the eyes, the sickness at stomach, and the general convulsion, as being all primarily dependant upon the irritation given to the intercostal nerve on the right side. And it may be observed, that, although both the *motores oculorum* were affected, yet the right eye was convulsed most violently.

From the symptoms attending this uncommon case, it is reasonable to conclude, that the three pins were all of them at the same time forced from the *oesophagus* into the *serrati* muscles on the right side, which immediately communicated an irritation, or impulse, to the intercostal nerve, from whence arose the pain in the side, and thence the sickness at stomach, and convulsions of the eyes and other parts.

But

But whatever caused the pain in the right side, upon the removal of the pins from the *æso-phagus*, that cause continued to act until all the three pins were discharged at the left shoulder, for so long did the pain in the right side continue.

The thickness of the two *ferrati*, the *rhomboides*, and *trapezius* muscles may be thought too great for pins five quarters of an inch long to penetrate all of them at the same time. But if it be observed, that one of the pins was discharged at a time when neither of the two others could be felt with the probe, it may be supposed, that one of the three passed into the *rhomboides*, and *trapezius*, whilst the two others remained in the *ferrati*, and there continued until the first was discharged at the *trapezius*; after which they took the same course, and were discharged at the same outlet.

Thus might we give a very probable account of this extraordinary case, had the pins been discharged at the right shoulder, but they were discharged at the left. By those who think that, the nerves communicating with one another, the cause and effect produced may be on opposite sides of the body, it may be said, that the pins might be forced from the *æso-phagus* into the muscles of the left side, notwithstanding the pain was felt in the right. This will not be generally allowed. Neither can I perceive any reason why a tumor exactly resembling that from whence the pins were afterwards discharged at the left shoulder, should arise upon the right, and disperse without coming to suppuration.

Since I drew out the above account, I have seen a case nearly similar to it, recorded in the Philosophical
 Transac-

Transactions, N^o 461. A small needle being lodged in a woman's left arm, about six inches below the shoulder, passed thence to her right breast, whence it was extracted many months after it first entered the body. About a month after the accident, she felt a pain above the place where the needle run in, which extended up her shoulder. It lasted there three or four days, and then returned by fits. About 17 weeks before the needle was extracted, she felt a pain at her stomach, was sick, and had reachings to vomit. These symptoms continued to afflict her (especially in the morning), until within two days of the needle being extracted, at which time she thought a pin had got into her right breast. This directed the surgeon to make an opening there, and he extracted the same needle that had entered at her arm from the part where the pricking pain was; after which she had never any return of pain in her breast, stomach, shoulder, or arm.

If, upon perusal of this case, you think it merits the attention of the curious, as corroborating the other, your recommendation of it to the Royal Society will be esteemed an honour to,

SIR,

Your much obliged,

humble servant

Gloucester, Sept. 1,
1768.

Dan. Lysons.

IV. *A Letter from the Honourable William Hamilton, his Majesty's Envoy Extraordinary at Naples, to Mathew Maty, M. D. Sec. R. S. containing some farther Particulars on Mount Vesuvius, and other Volcanos in the Neighbourhood.*

Villa Angelica, near Mount Vesuvius,
October 4, 1768.

S I R,

Read February 2,
1769.

I HAVE but very lately received your last obliging letter, of the 5th of July, with the volume of Philosophical Transactions.

I must beg of you to express my satisfaction at the notice the Royal Society have been pleased to take of my accounts of the two last eruptions of Mount Vesuvius. Since I have been at my villa here, I have enquired of the inhabitants of the mountain after what they had seen during the last eruption. In my letter to Lord Morton, I mentioned nothing but what came immediately under my own observation: but as all the peasants here agree in their account of the terrible thunder and lightning, which lasted almost the whole time of the eruption, upon the mountain only; I think it a circumstance worth attending to. Besides the lightning, which perfectly resembled the common forked lightning, there were many meteors, like what are vulgarly called falling stars. A peasant,

fant, in my neighbourhood, lost eight hogs by the ashes falling into the trough with their food: they grew giddy, and died in a few hours. The last day of the eruption, the ashes, which fell abundantly upon the mountain, were as white almost as snow; and the old people here assure me, that is a sure symptom of the eruption being at an end. These circumstances, being well attested, I thought worth relating.

It would require many years close application, to give a proper and truly philosophical account of the volcanos in the neighbourhood of Naples; but I am sure such a history might be given, supported by demonstration, as would destroy every system hitherto given upon this subject. We have here an opportunity of seeing volcanos in all their states. I have been this summer in the island of Ischia; it is about eighteen miles round, and its whole basis is lava. The great mountain in it, near as high as Vesuvius, formerly called Epomeus, and now San Nicolo, I am convinced was thrown up by degrees; and I have no doubt in my own mind, but that the island itself rose out of the sea in the same manner as some of the Azores. I am of the same opinion with respect to Mount Vesuvius, and all the high grounds near Naples; as having not yet seen, in any one place, what can be called virgin earth. I had the pleasure of seeing a well sunk, a few days ago, near my villa, which is, as you know, at the foot of Vesuvius, and close by the sea-side. At 25 feet below the level of the sea they came to a stratum of lava, and God knows how much deeper they might have still found other lavas. The soil all round the mountain, which is so fertile,

consists of stratas of lavas, ashes, pumice, and now-and-then a thin stratum of good earth, which good earth is produced by the surface mouldering, and the rotting of the roots of plants, vines, &c. This is plainly to be seen at Pompeii, where they are now digging into the ruins of that ancient city; the houses are covered, about 10 or 15 feet, with pumice and fragments of lava, some of which weigh three pounds (which last circumstance I mention to shew, that, in a great eruption, Vesuvius has thrown stones of this weight six miles, which is its distance from Pompeii, in a direct line); upon this stratum of pumice, or *rapilli*, as they call them here, is a stratum of excellent mould, about two feet thick, on which grow large trees, and excellent grapes. We have then the Solfaterra, which was certainly a volcano, and has ceased emptying, for want of metallic particles, and over-abounding with sulphur. You may trace its lavas into the sea. We have the Lago d'Averno and the Lago d'Agnano, both of which were formerly volcanos; and Attoni, which still retains its form more than any of these. Its crater is walled round, and his Sicilian Majesty takes the diversion of boar-hunting in this volcano; and neither his Majesty, or any one of his Court, ever dreamed of its former state. We have then that curious mountain, called Montagno Nuovo, near Puzzole, which rose, in one night, out of the Lucrine Lake; it is about 150 feet high and three miles round. I do not think it more extraordinary, that Mount Vesuvius, in many ages, should rise above 2000 feet; when this mountain, as is well attested, rose in one night, no longer ago than the year 1538. I have a project, next spring, of passing some days at Puzzole, and of dissecting

dissecting this mountain, taking its measures, and making drawings of its stratas; for, I perceive, it is composed of stratas, like Mount Vesuvius, but without lavas. As this mountain is so undoubtedly formed entirely from a plain, I should think my project may give light into the formation of many other mountains, that are at present thought to have been original, and are certainly not so, if their strata correspond with those of the Montagno Nuovo. I should be glad to know whether you think this project of mine will be useful; and, if you do, the result of my observations may be the subject of another letter.

I cannot have a greater pleasure than to employ my leisure hours in what may be of some little use to mankind; and my lot has carried me into a country, which affords an ample field for observation. Upon the whole, if I was to establish a system, it would be, *that mountains are produced by volcanos, and not volcanos by mountains.*

I fear I have tired you: but the subject of volcanos is so favourite a one with me, that it has led me on I know not how: I shall only add, that Vesuvius is quiet at present, tho' very hot at top, where there is a deposition of boiling sulphur. The lava that run in the Fossa Grande during the last eruption, and is at least 200 feet thick, is not yet cool; a stick, put into its crevices, takes fire immediately. On the sides of the crevices are fine crystalline salts: as they are the pure salts, which exhale from the lava that has no communication with the interior of the mountain, they may perhaps indicate the composition of the lava. I have done. Let me only thank you for

the kind offers and expressions in your letter, and for the care you have had in setting off my present to the Museum to the best advantage; of which I have been told from many quarters.

I am,

S I R,

Your most obedient,

humble servant,

W. Hamilton.

Received January 9, 1769.

V. *A Letter to Dr. William Watſon, F. R. S.
from the Hon. Daines Barrington, F. R. S.
on the Trees which are ſuppoſed to be indi-
genous in Great Britain.*

January 2, 1769.

DEAR SIR,

Read Feb. 9 and 16,
1769.

SINCE you ſent me the ſpecimen of ſuppoſed cheſnut, which was taken from the old hall of Clifford's Inn, I have been at ſome pains to examine into the authorities for the prevailing notion, with regard to this being an indigenous tree; as alſo with relation to ſome others, which are generally conceived to be of the native growth of Great Britain.

But, before I enter into other particulars, I ſhall venture to lay down ſome general rules, from which it may be decided, whether a tree is indigenous or not, in any country.

1. They muſt grow in large maſſes, and cover conſiderable tracts of ground; nor muſt ſuch woods end abruptly, by a ſudden change to other trees, except the ſituation and ſtrata become totally different.

2. If the tree grows kindly in copſes, and ſhoots from the ſtool, it muſt for ever continue in ſuch a wood.

wood, unless grubbed up with the greatest care ; nor is it then easily extirpated.

3. The seed of such tree must ripen kindly : nature never plants but where a succession may be easily continued, and in the greatest profusion.

Lastly ; Many places in every country must receive their appellation from indigenous trees which grow there ; as no circumstance is more striking, when a tract of ground is to be described or distinguished : hence so many towns, villages, and farms are named from the oak and ash, which are the most common trees of Great Britain.

When the instances of this are singular, it will prove directly the contrary ; as I hope to shew soon with regard to the chesnut and the box.

Having ventured to premise these general rules, by which it may be determined, whether a tree hath been planted by the hand of nature or not ; I shall now begin, by considering the proofs which are commonly relied upon, with regard to the Spanish, or sweet, chesnuts being indigenous in Great Britain.

And, first, the very name of Spanish seems most strongly to indicate the country from which it was originally introduced here, as much as if a particular species of oak was known in Spain by the name of the English oak.

There may be some doubts, perhaps, whether this tree is really a native of any part of Europe, as Pliny informs us, chesnuts were brought from Sardis to Italy, and that they were improved in taste by Tiberius, who took particular delight in cultivating them*.

* Pliny, lib. XV. cap. xxiii.

I have also been informed by you, that in Spain the chestnut trees, destined to produce the best fruit, are engrafted upon the wild chestnut, and that the French call the common sort *chataignier*, and the improved one *maronnier*.

Though so much hath been said of late, with regard to the excellence of this wood for building, I cannot, upon inquiry, find that it is greatly prized for this purpose either in Spain, Italy, or the South of France; but is chiefly valued for the fruit, which forms a considerable article of food for the inhabitants, as well as of exportation.

I likewise cannot hear that this tree is to be found in any considerable masses, till the traveller is at least two hundred miles to the south of Paris.

With us the nuts by no means ripen kindly, though I have sometimes eaten them very good from English trees.

In Scotland, neither the walnut nor chestnut produce good fruit, though there are some very fine and promising timber trees, of the latter kind, at the Earl of Bredalbane's, in the Highlands.

All these circumstances seem to afford a strong inference, that the Spanish chestnut cannot be a native of Great Britain; but I must now consider the proofs which are generally adduced to the contrary.

Mr. Miller (in his *Gardener's Dictionary*) hath endeavoured to prove, that the Spanish chestnut grew in great profusion to the northward of London, by a citation from Fitz-Stevens, which only implies, that there were large forests in the neighbourhood of the metropolis, without either the chestnut, or any other tree, being specified. “ Proxime patet foresta ingens,

“faltus numerofi, ferarum latebræ, cervorum, damarum, aprorum, et taurorum fylveftrium.”

Mr. Miller alfo mentions, that fome flumps of decayed chefnuts have been feen not far from the metropolis ; he does not, however, particularize the fpot, and it fhould feem therefore, that he had received this information from others.

Moft antiquaries fuppofe, that Old London was chiefly built with this kind of timber from thefe forefts ; there is not the leaft appearance, however, of any fuch tree at prefent within twenty miles of London, which may not be accounted for, as being of infinitely a more modern introduction than the time of Henry the Second, when Fitz-Stevens wrote.

I remember the having once been prefent myfelf, when a wager on this head was won ; it being fuppofed that a fmall fpecimen of a beam, from a very ancient houfe in Chancery-lane, was of this wood ; which turned out to be nothing but common oak.

When you, therefore, lately put into my hands another fuch fpecimen of fuppofed chefnut, from the old hall of Clifford’s Inn, I knew it immediately to be only the common oak.

As I had, however, at that time, an opportunity of proving this to a demonftration, by fending into the country for part of an oaken beam from a very ancient ftale, and alfo a piece of Spanifh chefnut, which grew near ; I fhall, for your further fatisfaction, fend three fpecimens, which you will compare where the wood hath been cut tranfverfly, and where they are marked with ink at top and bottom.

The large irregular piece, marked *CZ.*, is the fuppofed chefnut from Clifford’s Inn.

The

The specimen, marked O, is from an oaken beam of an ancient stable.

The specimen, marked C, is from a Spanish chesnut.

I think it must immediately appear to any one, on inspection, that the specimens C/ and O agree in the grain and texture of the wood, and that specimen C is evidently of a different kind.

Upon weighing also the specimens C and O, which are exactly of the same size, the oak turned out to be heavier than the chesnut, by one fourth.

With regard to this latter difference between the two woods, it may be proper to inform you, that the specimen of chesnut was taken from a young tree; the grain of the oak must have therefore been closer than that of the chesnut; but, on the other hand, it must be recollected, that there must have been a very considerable evaporation from the oaken beam during a long course of years.

Dr. Ducarel, in his *Anglo-Norman Antiquities**, hath inserted a note of some length, to prove, that Old London was not only built with chesnut timber, but that there still continues a large tract of chesnut woods near Sittingbourn in Kent, which he conceives to be a full demonstration, that this tree is indigenous in England.

I had no sooner read this account, than I determined to examine these woods myself, as well as what trees might be found in their neighbourhood.

The result of a very minute inspection of them is, that I found those parts which consist of Spanish

* P. 96.

chestnut to be planted in beds or rows, about five yards distant from each other; nor are there any scattering trees to introduce them, which is what must be expected near woods of natural growth.

I shall now proceed to answer Dr. Ducarel's other arguments, with regard to the Spanish chestnut's being an indigenous tree in this country.

He first mentions a grant *Decime Castanearum in Dean*, which he supposes to mean the forest of Dean.

Upon looking into Spelman's Index Villaris, I find no less than two-and-twenty towns and villages which bear the name of *Dean*. Why, therefore, it should mean the Forest, rather than any of these places, is not so obvious; especially when, considering the vast tract of ground included within this forest, the grant must have been of so very extensive a nature.

Supposing it to be the tithe *Castanearum* in any particular parish, it will amount to no more than a grant of the tithe of walnuts would do, which we know to be a tree originally of foreign growth.

The fruit of a small number either of walnuts or chestnuts is very valuable, if near a considerable town. I have been informed, that a grove, not exceeding an acre, of the latter, at Beachworth Castle in Surrey, hath sometimes produced upwards of ten pounds, at the London market, when the season happened to be favourable, and the nuts ripened kindly.

I should suppose that this grove of chestnuts, from their size, may be about two hundred years growth; and they already begin to decay very much at the tops, being what the woodwards term stag-headed.

If

If it be still contended, however, that this grant of tithe includes the whole forest of Dean, I have been in almost every part of it, and can take upon myself to say, that there are not the least vestiges of any such tree at present.

Dr. Ducarel next relies upon a manor in the neighbourhood of Sittingbourne being called Chastenyne or Castenye, from the circumstance of its being supposed to be amongst chesnut woods.

This, however, is a single instance of such a name to any place in England; and therefore the chesnuts being indigenous can be no more inferred from it, than that box naturally grows in this country, from the name of Box-hill, in Surrey.

Now we happen to know that this hill was so called from an Earl of Arundel's having introduced this tree there, in the time of James or Charles the First*; and, from many circumstances, I should suppose that the chesnut plantations near Sittingbourne are not of a much more ancient date.

Dr. Ducarel then mentions two very fine chesnut-trees, which grow at Hagley in Worcestershire; this, however, only proves, that the owner of that estate, some time ago, might think it worth while to plant them either for their beauty or their fruit.

* " This place (viz. Box-hill) was first planted by that famous antiquary (the Earl of Arundel), with box wood, designing to have built a house there; but want of water made him alter his resolution, and build one at Albury, hard by; " now belonging to the Earl of Aylesford." *Journey through England*, vol. I. printed in 1722.

See also the Article *Box-HILL*, in an *Account of the Environs of London*, printed for Doddley.

The oldest tree we have any account of, perhaps in Europe, is a Spanish chesnut which grows in a court at Tortworth in Gloucestershire: it is supposed by Evelyn and Bradley to have been planted in the time of King John, from mention of it in deeds of that antiquity.

I have, however, procured more accurate information from Lord *Ducie*, to whom this tree belongs; and find that the notion of its great age rests merely upon a very uncertain tradition.

But although we should suppose it to have been planted in the time of King John, it affords no stronger argument of the tree's being indigenous, than those mentioned by Dr. Ducarel to grow at Hagley; especially as there are no straggling chesnuts to be found in the neighbourhood of either of these places.

In further proof that the chesnut formerly grew in this country, we are told, that the roof of Westminster-hall, Boston church in Lincolnshire, and many others, consist of this wood; not because any one hath found it to be so upon examination, but because there are no cobwebs upon such roofs.

Sometimes also, to account for spiders not harbouring in them, it is supposed that the timber is not English, but Irish oak; in short, recourse is had to any extraordinary and uncommon material, to solve the singularity of there being no cobwebs on these roofs.

Having examined several ancient ciellings with regard to this circumstance, I take the cause of the spiders not resorting to many of them to be the following:

This

This insect is known to subsist chiefly on the small flies which he surprizes in his nets; the consequence is, that he will no more be at the trouble of spreading his web where flies cannot be expected, than a fowler will lay his nets in a place where there is no resort of birds.

It is believed that few of the fly tribe are found at any great height from the ground, as they may be supposed to prey upon still smaller microscopical insects, which would be the sport of the winds at any considerable elevation. They are not therefore formed by nature for a high flight.

If one may be allowed also to argue from what is observed with regard to the smallest birds; neither the European wren nor the American humming-bird are ever seen upon any thing higher than a shrub.

Besides this, no fly is scarcely ever to be found but where there is a good deal of light and sunshine; consequently a wide wooden roof (be it of what material it may) is the most improper place that the spider can lay his snares in.

If such roof therefore is dark, though it is at the same time very low, no flies will haunt it; for a proof of which I may refer you to the cloisters at Lincoln, or any gloomy cellar, though it may be above ground, and have windows which give it a certain degree of light.

Hence also spiders webs are more common on whitewash than on wainscot, especially if it be painted of a dark colour.

Having dwelt thus long upon the point of the Spanish chesnut's being indigenous or not, I shall

now trouble you with some observations relative to the Pine commonly called the Scotch Fir, which certainly is not to be found in any part of England at present, except where the plantation appears most evidently to be of modern date.

Cæsar, indeed, informs us, that no sort of fir was to be seen in this country at the time of his invasion.

It is well known that he made no very far advanced marches ; and his observations are, perhaps, more to be depended upon with regard to military operations, than what might rather engage the attention of a botanist.

There are, however, so many well-attested facts, both by Camden and others*, of firs being found at a very considerable depth under the surface of the ground, that one cannot withhold one's assent to them, extraordinary as it may appear at present, when throughout England we have no such trees, which afford the least grounds to contend that they are of indigenous growth.

If these indisputable facts could want the addition of my poor testimony, I happened to see near Loch Rannoch, and in other parts of the Highlands of Scotland, subterraneous firs, which had been lately dug out.

I procured a labourer to chip off some parts of these trees, which smelt most strongly resinous ; there could be no doubt therefore, from this circumstance, but that they were firs, as well as from the grain of

* See Camden in Lancashire, and Phil. Transf. N° 67, where such subterraneous firs are said to be found in great quantities in the island of Axholm in Lincolnshire.

the wood : the poor people in those parts use small pieces of them for candles.

There were, however, no fir woods near any of those places, in the Highlands, where I happened to see these subterraneous trees ; and, indeed, the indigenous ones are by no means so common as is generally apprehended.

Though what I have last mentioned may, perhaps, make many imagine, that the timber found under ground must have been some other tree, which still continues to grow in the neighbourhood ; yet I think there may be two causes assigned, why these bog-firs may be found in places where there is no such tree at present.

The first is, that no pine or fir ever shoots from the stool ; and the second, that, being a resinous wood, it is very easily set on fire by lightning, after a dry summer ; and thus whole tracts of them may be destroyed without their revegetating.

I was, indeed, informed by an old man at Ranoch-Bridge, that his grandfather used to mention a tradition of the fir wood in that neighbourhood having continued burning for a considerable time, and that the Irish came over to see the conflagration.

A wood of this kind is still growing near the western end of Loch Ranoch, but it is seven or eight miles from the place where I saw the subterraneous trees, near which there was scarcely any other wood but birch.

There seems to be little doubt, therefore, that the fir was formerly an indigenous tree in the northern parts of England ; nor does this contradict any of the

rules which I have ventured to lay down, as they have been found in great masses under ground, and their not continuing to grow in the same spot or neighbourhood hath been endeavoured to be accounted for.

As I travel a good deal during the summer, and attend to matters of this sort, I shall now venture to mention some other trees, which do not seem to be indigenous, though they are commonly conceived to be so, as well as by some great botanists, who have treated of English plants and trees.

I cannot think that the elm, which we see every where, is indigenous. My reasons are, that I have never seen it out of a hedge-row, avenue, or clump, though it is a tree which shoots vigorously from the stool: I have likewise never observed any seeds on this tree, though you have lately informed me, that they stand on very short footstalks, and that the blossom in the spring is of a pale red.

Upon looking into Mr. Miller's Dictionary, I find he likewise asserts that this tree bears seed; but at the same time mentions its being difficult, if not impracticable, to supply the nurseries from it; which shews that it scarcely ever comes to maturity in this climate.

The Wych (or broad-leaved) elm, however, is certainly of natural growth in this country; though it is more common in Scotland, and the northern parts of England, than in the southern counties.

For the same reasons, I cannot allow the lime to be indigenous, though in some years the seed becomes mature.

The greater part of the limes, which we now see in every part of England, have been planted since the time of Charles the Second, and were introduced by a French gardener, whose name was Le Notre, at the same time with the horse-chestnut.

There are, however, at More-Park in Hertfordshire, six or seven of these trees, which appear to be the growth of some centuries; so small a number, as well as two or three limes of great antiquity on the banks of the river Neath in Glamorganshire, only prove, that they were planted by some gentleman, either for variety, or perhaps for the fragrancy of the flower. I should indeed rather suppose, that we owe most of the foreign trees which may have been introduced into England some centuries ago, to the alien abbots and priors, who, on special occasions, sometimes visited their benefices in this country.

The greater Maple (or Sycamore, as it is improperly called) is certainly a foreign tree, though very common in Scotland, the northern counties of England, and some parts of Wales.

I never saw the tree but in a hedge-row, avenue, or clump; it must be admitted, however, that its seed comes to its full perfection in almost every year.

I have already mentioned a reason for the box not being considered as indigenous; to which I must now add, that no such place as Box-hill is to be found in Saxton's maps, which were completed in the reign of Queen Elizabeth; nor is it taken notice of by Gerard, in his Article Box, who botanized so much in the neighbourhood of London.

We should, indeed, find great profusion of this shrub in Berkshire, as *Aster Menevensis* hath derived the name of that county from the word *Berroc*: I could never either see, or hear of, box in any part of it, except where it had been used in a *parterre*, to separate the beds of flowers.

I have likewise looked into Benson's Saxon Vocabulary, nor can I find any such word as *Berroc*. Mr. Lye's new Saxon Dictionary furnishes also no such Article; on the contrary, he derives *Bapocrepte* a quadam nudâ quercu in foresta de Vindeyroupe ad quam solebant provinciales convenire. He cites for this *Joh. Bromton* 801.

I shall now mention some trees and shrubs, which I have doubts whether they are natives of Great-Britain or not, though they are so considered by the Botanists; I cannot pretend, however, to be so positive as in some of the former articles.

I never saw the Yew where it grew in large masses, or appeared to have been sown by the hand of nature. The most considerable numbers which I have happened to meet with are on some of the Surrey hills: these, however, scarcely in any instance, cover more than an acre of ground.

If I should be right in supposing that this tree is of foreign growth, it may then be, perhaps, contended, that we have no ever-green tree or shrub which is indigenous, except the Holly, the Juniper, and the Ivy.

Every church-yard, indeed, proves that this tree hath been for many centuries introduced into England; it seems, however, very extraordinary that we should have no account when, or for what purpose, this

this so very general a practice hath so long prevailed with us.

As far as I can be informed, after very diligent inquiries, it is peculiar to England; and I never saw but one yew-tree in a Scotch church-yard; which was of so extraordinary a size, that you will forgive me, I am sure, for mentioning it, though it hath no relation to the main purpose of my letter.

It continues to vegetate at present in the church-yard of Glen-Lyon, a valley that takes its name from a river which runs through it, and empties itself into the Tay not far eastward from Taymouth, a most capital and picturesque seat of the Earl of Bredalbane's.

I measured the circumference of this yew twice, and therefore cannot be mistaken, when I inform you that it amounted to fifty-two feet. Nothing scarcely now remains but the outward bark, which hath been separated by the centre of the tree's decaying within these twenty years. What still appears, however, is thirty-four feet in circumference.

This, therefore, is, perhaps, the largest tree we have any account of; as the great chestnut at Tortworth, in Gloucestershire, was only 51 feet in circumference, when measured very accurately forty years ago by Greening, the father of the present gardener of that name.

To the catalogue of doubtful trees, I must also add the Abele, or White Poplar, having never seen it growing according to the rules which I have

ventured to lay down with regard to indigenous trees*.

I have likewise my suspicions with regard to the Privet and Spindle tree; but these I must submit to your more accurate observations; and am,

Dear Sir,

Your most faithful

humble servant,

Daines Barrington.

* I believe it hath hitherto escaped the botanists, that it is only in the last shoots of this tree, that the glossy and striking white is to be seen on the reverse of the leaf.

VI. *An Account of a Case in which the upper Head of the Os Humeri was sawed off, a large Portion of the bone afterwards exfoliated, and yet the entire Motion of the Limb was preserved. By Mr. White, Surgeon, at Manchester. Communicated by Mr. Watson, F.R.S.*

Read February 9,
1769.

EDMUND POLLIT, of Sterling, near Cockey Moor, in this county, aged 16, of a scrophulous habit of body, was admitted into the Manchester infirmary, April 6, 1768. The account I received with him was, that he had been suddenly seized, about a fortnight before, with a violent inflammation in his left shoulder, which threatened a mortification, but at last terminated in a large abscess, which was opened with a lancet a few days before his admission. The orifice was situated near the *axilla*, upon the lower edge of the *pectoralis major*; and through it, I could distinctly feel the head of the *os humeri* totally divested of its burfal ligament. The matter, which was very offensive, and in great quantity, had made its way down to the middle of the *humerus*, and had likewise burst out at another orifice just below the *processus acromion*; through which the head of the *os humeri* might easily be seen. The whole arm and hand were
swelled

swelled to twice their natural size, and were entirely useless to him ; he suffered much pain, and the absorption of the matter had brought on hectic symptoms, such as, night-sweats, *diarrhea*, quick pulse, and loss of appetite, which had extremely emaciated him.

Under these very dangerous circumstances, there seemed no resource but from an operation. The common one in these cases, that of taking off the arm at the articulation with the *scapula*, appeared dreadful both in the first instance and in its consequences. I therefore proposed the following operation, from which I expected many advantages ; and performed it on the 14th of the same month.

I began my incision at that orifice which was situated just below the *processus acromion*, and carried it down to the middle of the *humerus*, by which all the subjacent bone was brought into view. I then took hold of the patient's elbow, and easily forced the head of the *humerus* out of its socket, and brought it so entirely out of the wound, that I readily grasped the whole head in my left hand, and held it there till I had sawn it off (see Tab. I. Fig. 1.), with a common amputation-saw, having first applied a pasteboard card betwixt the bones and the skin. I had taken the precaution of placing an assistant on whom I could depend, with a compress just above the clavicle, to stop the circulation in the artery, if I should have the misfortune to cut or lacerate it ; but no accident of any kind happened, and the patient did not lose more than two or three ounces of blood, only a small artery which partly surrounds the joint being wounded, which was easily secured.

He

He was remarkably easy after the operation, and rested well that night; the discharge diminished every day, the swelling gradually abated, his appetite returned, and all his hectic symptoms vanished. In about five or six weeks, I perceived the parts, from which the bone had been taken, had acquired a considerable degree of firmness, and he was able to lift a pretty large weight in his hand. At the end of two months, I found that a large piece of the whole substance of the bone, that had been denuded by the matter, and afterwards exposed to the air, was now ready to separate from the sound, and with a pair of forceps I easily removed it (Fig. II.). After this exfoliation, the wound healed very fast; and on August 15th he was discharged perfectly cured. On comparing this arm with the other, it was not quite an inch shorter; he has the perfect use of it, and can not only elevate his arm to any height, but can likewise perform the rotatory motion as well as ever. The figure of the arm is no ways altered; and from the use he has of it, from its appearance to the eye, and to the touch, I think I may safely say, the head, neck, and part of the body of the *os humeri* are actually regenerated.

I did not make use of any splints, machine, or bandage, during the cure, to confine the limb strictly in one certain situation, nor was his arm ever dressed in bed, but sitting in a chair, and, as soon as he could bear it, standing up, with his body leaning forwards, to give room for applying the bandages, which were no more than just necessary to retain the dressings; and to this method I attribute the preservation of the motion of the joint, which could not have

VOL. LIX. G been

been so well effected any other way ; but the joint, in all probability, would have remained stiff, and formed an *anchylosis*, if it had not been allowed to play about.

R E M A R K S.

Though from this operation I hoped for many advantages preferable to the amputation of the limb at the *scapula*, yet my most sanguine expectations fell greatly short of the success attending it. I did not flatter myself with the hopes of a moveable joint, or that the length of the limb would be so nearly preserved, when there was a loss of above four inches of the whole substance of the bone without any other bone to support it as in the leg and fore-arm ; and where the dreadful condition of the arm at the time of the operation prevented me from making use of any machine to keep it extended *. But I suppose the weight of the arm was in this case in some measure sufficient to counterbalance the contractile power of the muscles, for his arm was only suspended by a common sling, and the patient not at all confined to his bed.

I could not help being surprized to find so much strength and firmness in the parts as evidently shewed a regeneration of the bone before the lower part had

* After the extraction of three inches and ten lines of the *os humeri*, M. Le Cat made use of a machine to keep the upper and lower pieces of the bone at their proper distances. He has given a description of the case, and a figure of the machine, in vol. LVI. of the *Philos. Transact.* p. 270.

exfoliated, or even before it had begun to loosen. This osseous matter could not proceed from the *scapula*, the glenoid cavity of that bone not being divested of its cartilage; could it then possibly escape from the end of the sound bone, before the morbid part had begun to separate from it? or are there any vessels that could convey the bony matter, and deposit it in the place of what had been removed *? These are points that I will not pretend to decide absolutely, but I am much inclined to the latter opinion †.

* Mr. Gooch, in his volume of Cases and Practical Remarks, relating the case of a compound fracture of the leg, where a very considerable portion of the *tibia* was sawed off, says, “ In about “ three weeks I was sensible, as were also several surgeons whom “ curiosity led to see so uncommon a case, that the substance “ which grew in the space of five inches entirely void of bone, “ had acquired in the middle only a greater degree of solidity “ than flesh; which circumstance not agreeing with the general “ received notion of the generation of *callus*, we proved beyond “ dispute, with a sharp pointed instrument, and we observed that “ the ossification was gradually formed from that central point, “ which was considerably advanced before any exfoliation was “ cast off the ends of the divided bone. In less than four “ months, the whole space was so well supplied with the *callus*, “ or rather new bone, that he was able to raise his leg when the “ bandage was off, without its bending.”

Cases and Remarks, new Edit. p. 287.

† In universum in sanguine materies est apta producendo ossi, quæ adeo frequenter in cellulosum spatium intimum, interque convexam superficiem membranæ intimæ arteriarum, concavamque membranæ musculosæ extremitatem effunditur, et casciosa primo, inde callosa, quasi coriacea, demum ossæ squamæ fit simillima.

Halleri Elem. Physiolog. tom. VIII. p. 316.

Calli in ossibus non fracturas solas, sed amissâ integrâ ossa farcientes, fiunt ex liquido glutinoso, pulsu proximarum arteriarum compacto, &c.

Halleri Primæ Linææ, p. 148.

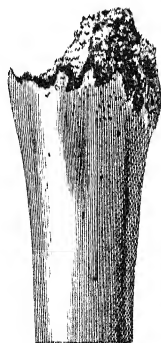
See further Haller's Pathological Observations, Obs. xlvii.

Is it not probable that there is a regeneration of the cartilage as well as the bone? for it is well known to every-body conversant in anatomy, that the ends of some bones, which are joined to no others, are covered with cartilages; but these are never wanting on the ends, and in the cavities of such bones as are designed for motion; and I cannot see in this case how the motion could be preserved so complete without a cartilage; and indeed without a bursa ligament, or something analogous to it, to contain the *synovia*, and keep the bone in its place.

As this is the first operation of the kind that has been performed, or at least made public, I thought the relation of it might possibly conduce to the improvement of the art. That ingenious surgeon, Mr. Gooch, has indeed related three instances of the heads of bones being sawed off in compound luxations. In one of these cases the lower heads of the *tibia* and *fibula* were sawn off, in another that of the *radius*, and in the third that of the second bone of the thumb; but these were, in many respects, different from the present case.

I believe it will seldom happen that this operation will not be greatly preferable to the amputation of the arm at the *scapula*, as this last is generally performed for a *caries* of the upper head of the *os humeri*; and as the preservation of a limb is always of the utmost consequence, and what every surgeon of the least humanity would at all times wish for, but particularly where (as in this case) the whole limb and its actions are preserved entire, the cure no ways protracted, and the danger of the operation most undoubtedly less. For though amputation is often indispensably necessary,

Fig. 1.



a

Fig. 2.

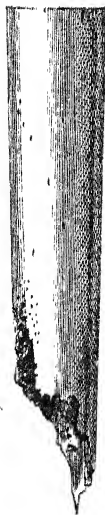
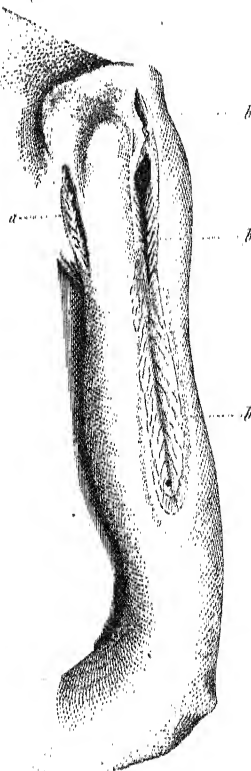


Fig. 3.



fary, and frequently attended with little danger or inconvenience when only part of a limb is removed ; yet where the whole is lost, the danger is greatly increased, and the loss irreparable.

I have frequently performed this operation on dead subjects, and where the parts had not been diseased, and never found any difficulty ; and from the dissection of the parts had no reason to doubt of success in a living subject, where the ligaments and muscles are more supple, and the matter, by insinuating itself betwixt the bone and integuments, has made less dissection necessary.

I have likewise, in a dead subject, made an incision on the external side of the hip joint, and continued the incision down below the great *trochanter*, then cutting through the burfal ligament, and bringing the knee inwards, the upper head of the *os femoris* was forced out of its socket, and easily sawed off ; and I have no doubt but this operation might be performed upon a living subject with great prospect of success.

The Royal Academy of Surgery at Paris proposed for a prize question, Whether amputation of the thigh at its articulation with the *os innominatum* was ever adviseable : but was I under a necessity of performing this operation, or that which I have been describing, I should not hesitate a moment which to prefer.

C. White.

TAB. I. FIG. I.

That part of the bone which was sawed off.

a. The head of the bone corroded by the matter.

FIG.

FIG. II.

The piece of bone which exfoliated. Both pieces together were five inches in length, four of which were of the whole substance.

FIG. III.

The arm as it appeared after the wound was healed.

a. The cicatrix of the first incision.

b.b.b. The cicatrix of the incision made to bring out the head of the bone.

N. B. The drawings from which this plate was engraved were made by Mr. Aikin, my pupil, who attended at the operation, and during the whole cure.

Received February 9, 1769.

VII. *Letters from the Rev. Dr. William Borlase, F. R. S. Rector of Ludgvan in Cornwall, to Charles Morton, M. D. F. R. S. and from Mr. Rosewarne, of Truro, to Dr. Borlase; giving an Account of a Specimen of Native Tin found in Cornwall, and now deposited in the Museum of the Royal Society.*

Ludgvan, Jan. 30, 1769.

S I R,

Read February 16, 1769. **P**ERHAPS you may not have forgot that, in the year 1765, I sent a specimen of native tin, to be deposited in the Royal Society's Museum; and though the account of it, published in the Transactions of the year following, was not such as I wish, yet I am steadily intent on paying my duty to the Society, and obviating (as far as lies in my power) all doubts relating to natural knowledge.

Last post I received a letter from Mr. Rosewarne, of this county, with an account of his having met with another specimen of native tin; and I send it you inclosed, for your inspection, to be returned at
your

your leisure. This young gentleman is a considerable merchant, especially versed in tin affairs, with a great share of quickness and understanding in the fusion of metals; and I refer it to you, whether it may not be proper to introduce him at the next meeting of the Royal Society, by your means, and request him there to shew that rare specimen which he carries with him to town.

He, indeed, intends (you'll find by his letter) to bring it back with him; but if this be only to let me have the inspection of it, and, through my hands, to deposit it in your Museum, I shall easily dispense with every ceremony of that kind; and I write him by this or next post, that (as it is small, and may miscarry, or be mislaid in such long journeys) I intreat him to let it rest, for the satisfaction of the doubtful, in your Museum; and, as I hope to prevail, let it remain with the sample I sent, but in Mr. Rosewarne's name.

As every acquisition in natural history will, I know, be very acceptable to you and the gentlemen of the Society, for whom I have the greatest respect, I make the less apology for giving you this trouble, and remain,

S I R,

Your most obedient servant,

William Borlase.

Received

Received February 9, 1769.

To the Rev. Doctor William Borlase.

Rev. Sir,

Truro, Jan. 27, 1769.

Read February 16,
1769.

I HAVE the pleasure to acquaint you, that I have at last met with a specimen of native tin, which is so evidently so at first sight as not to admit of the least doubt or objection. The description of it is this: some streamers in the parish of Luxilion brought in a parcel of tin ore to a blowing-house I am concerned in at Sthurtle; amongst the parcel was a great number of tin diamonds of a most beautiful nature, of the rozin kind; one was eminently superior to the rest, being almost transparent, and seemed to have something in the center which, through the stone, looked like gold. This induced me to break the stone; which was no sooner done than I found it to be native tin, in the very center of the diamond. The specimen is so small, that I am at a loss which way to send it for your inspection, for fear it should be lost. I shall set out for London on Monday next, and intend carrying this curiosity with me. I'll not leave it behind me; but when I come back you shall see it, and through you deposit it with the Royal Society, for the satisfaction of the curious. I am,

Rev. Sir,

Your obliged and faithful

humble servant,

Henry Rosewarne.

VIII. *An Account of an Essay on the Origin of a natural Paper, found near the City of Cortona in Tuscany. In a Letter from John Strange, Esq; F. R. S. to Mathew Maty, M. D. Sec. R. S.*

DEAR SIR,

Read February 16,
1769.

MY letter to Mr. Coltellini, secretary to the Botanical Academy of Cortona, concerning the origin of a natural paper found in the neighbourhood of that city (which, with some specimens of paper, you obligingly presented to the Royal Society in my name), being written in a foreign language, and but little known; I have thought proper to give you the following short account of it, together with some additional remarks, which I have made since its publication.

In August 1763, some low grounds, in a farm about four miles south-west of Cortona, which had been flooded, were found covered with a substance very much like a finer sort of common brown paper. A circumstantial account of the fact was communicated to the public the September following, in a letter from the said Mr. Coltellini to Dr. Lami, professor of theology at Florence.

This account surprised, and excited the curiosity of the naturalists in Italy; and various were the conjectures

jectures offered upon the occasion. The prevailing opinion, however, attributed the formation of this paper to a casual aggregate of the fibres of different kinds of filamentous plants, collected together by the waters, and left on the surface of the ground at their retreat. This supposition seemed plausible enough, since such a mechanism could be produced only by filamentous plants; most of which are commonly the spontaneous productions of such low, marshy ground. But upon considering that, in the paper manufactures of different countries, various degrees and methods of maceration are requisite, according to the respective qualities of the fibres of different plants; it appeared to me very difficult to conceive, that a paper of so delicate and uniform a texture as that of Cortona should owe its origin to so complicated and remote a cause.

To bring the matter in question to a more certain issue, I therefore thought it necessary to examine the threads of this paper with a good microscope; and, agreeable to the opinion I had entertained, found them to consist of mere filaments of the common species of *Conferva*, without the intervention of any other plant whatsoever. It was easy enough to ascertain the identity of the *Conferva*, the filaments of which it is composed being of a peculiar structure, and very different from those of any terrestrial plant; besides, as they are solitary in their natural state, they undergo no other alteration by the above mechanism, than the loss of the *parenchyma* that invests them, the structure of the filaments themselves remaining as perfect as ever.

To confess the truth, I was but very superficially acquainted with this species of *Conserva* till I had made the above discovery; since the descriptions of it, which we find in the books of botany, by no means afford an adequate idea of the structure of the plant. Dillenius (1), in his description of it, pretending to correct Pliny, for a supposed impropriety in the term *fistulosæ densitatis*, says, that there is no cavity observable either in this or other larger species of *Conserva*, excepting, perhaps, in his *Conserva dichotoma* (2); in which he is certainly mistaken; since the filaments of the common *Conserva*, when examined with a good microscope, evidently appear to be capillary tubes divided at equal distances by parallel *septa* or diaphragms, exactly like the 25th species of the same *genus* in Dillenius's Tables. Pliny's (3) epithet, therefore, so far from being improper, is a real characteristic of the thing in question.

As the systematical botanists generally take their leading characters from the external figures of plants, we need not be surprised to find them inaccurate in their descriptions of the smaller tribes; more especially as they neglect the use of proper glasses, by which alone they can acquire a knowledge of them. Dillenius and Linnæus himself have both been led into mistakes, from this omission. The former, in the preface to his *Historia Muscorum*, confesses, that he made use of common glasses only, in order that the figures of the smaller plants, which he was to

(1) Hist. Musc. Gen. 1. Ord. 1. Sp. 1, 2.

(2) Ib. Gen. 2. Sp. 9.

(3) Hist. Nat. lib. XXVII. cap. viii.

represent in his Tables, might not deviate too much from the natural appearance of the plants themselves to the naked eye : and it is pretty evident that the glasses he used were but of moderate powers, since, besides other mistakes, they left him quite undetermined whether his 4th and 5th species of *Conferva* had ramifications or not, though this very distinction forms a separate series in the first *Ordo*. Linnæus's (4) generical character of this plant is certainly less defective than that given by Dillenius, inasmuch as he takes notice of the tubercula omitted by the former, and calls the fibres of the *Conferva capillary*; but as he does not expressly say, whether these fibres are tubes or not, and takes no notice of the *septa* or diaphragms distributed at equal distances along them, I apprehend that he equally neglected examining the plant with proper glasses. Perhaps he adopted the term *capillaris* from professor Van Royen's Synonyme, which he quotes; especially since, in his divisions and specific characters of the *Conferva*, he has fallen into the same mistakes with Dillenius, whom he chiefly followed in his class of the *Cryptogamia*.

If the systematical botanists have not therefore acquired an adequate knowledge of the structure of the minuter *Confervæ*, by neglecting to use proper glasses, their descriptions of these plants must necessarily be imperfect.

The specimens of paper, which I sent you with the copy of my letter, are,

First, A specimen of the natural paper of Cortona.

Secondly, An artificial paper made of the same substance with the natural paper of Cortona; which

(4) Gen. Plant. Cl. 24.

substance

substance I prove (5) to be the common Conferva; but as the plant, by mistake, was not kept long enough in maceration, the parenchymous matter, which ought to have been separated from it, is in part still remaining, and gives the paper a greenish colour, besides making it very brittle.

Thirdly, A specimen of a much better and stronger paper made of the same Conferva, by Sir Alexander Dick, baronet, near Edinburgh; and I remember seeing others of the same sort, but of inferior quality, made by Mons. Guettard, of Paris; an account of which has been already printed (6).

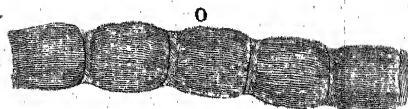
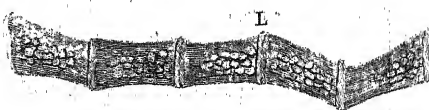
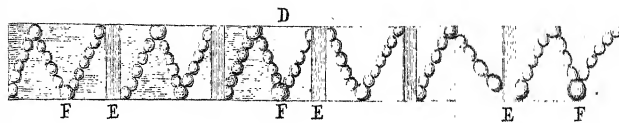
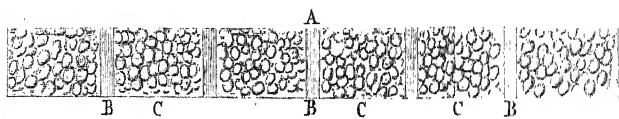
Fourthly, A specimen of another artificial paper, which I made of the *Genista Juncea* macerated in warm water, and prepared afterwards in the common manner. I do not recollect, that this substance was ever tried before; neither is the *Genista Juncea* inserted in the list of filamentous plants published by Monsieur De la Lande (7). This anecdote, though Vitruvius (8) recommended it for similar uses so many centuries ago, was new to Monsieur De la Lande, when I had the opportunity of communicating it to him at Paris on my return from Italy in 1764.

These matters are, I confess, of more speculation and curiosity than use; but as they have given occasion, in the course of my inquiry, to some physical and botanical remarks, I thought they might not prove wholly unacceptable to the learned members of the Royal Society; and should think myself very happy, if other gentlemen, of greater knowledge

(5) See the Table of References to the Figures.

(6) Journ. Œcon. Avril, 1761. (7) Art de faire du Papier.

(8) Archit. lib. VII. cap. iii.



and abilities than myself, should be hereby encouraged to pursue the above hints, in order to obtain a more accurate knowledge of a genus of plants hitherto but imperfectly described, and perhaps less known than any other of the *Cryptogamia* class. I am,

Dear Sir,

Your most obedient,

and humble servant,

John Strange.

Appearances of the *Conferva Plinii*, viewed in different states with the microscope; with references to the plate. See Tab. II.

- A. *Conferva Plinii*, in its natural vigorous state, of a deep green colour; taken from the fountain in the middle of the botanical garden at Florence.
- B. B. B. Transverse membranes or diaphragms, apparently of a spongy nature, and transparent.
- C. C. C. Parenchyma which gives the deep green colour to this plant, replete with globular appearances, like air-bladders.
- D. *Conferva Plinii*, of a yellowish green colour; taken from a fountain in Marquis Grifoni's garden at Florence. This plant was in the first stage of putrefaction, a great part of its parenchyma being dissolved and separated from it: hence its degradation of colour.

E. E. E. The

- E. E. E. The transparent diaphragms.
- F. F. F. Appearance of the parenchyma with fewer, but apparently regular vesicles of air.
- G. *Conferva Plinii* A. dried between two pieces of glass, and thereby contracted at the joints. The parenchyma of the plant had stuck these pieces of glass so fast together, that they were with difficulty separated.
- H. *Conferva Plinii* D. dried between two pieces of glass. The apparently longitudinal fibres are nothing but foldings of the membranous tube ; for, on moistening the plant, and moving the pieces of glass in different directions, these foldings increase or decrease arbitrarily.
- I. K. L. Appearances of *Conferva Plinii*, taken from three different places in the canals about Pisa, afterwards steeped for fifteen days in common water, then extended upon paper to dry, and put a second time into water for an hour.
- M. Appearance of a thread of the natural paper of Cortona, with the diaphragms NN appearing opaque.
- O. A thread of the same paper, steeped for an hour in common water.

Received February 9, 1769.

IX. *Experiments on the lateral Force of Electrical Explosions.* By Joseph Priestley, L.L. D. F. R. S.

Read February 23,
1769.

BEING informed, in accounts of damages done by lightning, of persons and things being removed to considerable distances, without receiving any hurt, I was excited to try whether I could produce similar effects by electricity. All the other known effects of lightning had been frequently imitated by the application of this power; but I do not know that this effect has ever been so much as taken notice of by any electrician. The experiments I presently found to be very easy; and I think it not difficult to ascertain the cause of this striking effect, and the manner in which it is produced.

If pieces of cork, wood, powder of any kind, or any light bodies whatever, be placed near the explosion of a jar, or battery, they will not fail to be moved out of their places, at the instant of the discharge. If the explosion of a large battery be made to pass over the surface of animal or vegetable substances, in the manner described in the printed account of my experiments, and large corks be strewed along, or near the path intended for it, it is surprizing

to observe with what violence they will be driven about the room; and this dispersion is in all directions from the center of the explosion; and it makes no difference whether the rods, between which it is made, be sharp-pointed or otherwise.

The effect of this lateral force is very remarkable in attempts to fire gunpowder in electrical explosions. If the gunpowder be confined ever so close in quills or cartridges, and they be held fast in vices, yet, when the explosion is made in the center of them, it will sometimes happen, even when a wire has been melted in the midst of the powder, and the fragments have been seen red-hot for some time in different parts of the room, that the powder has not been fired, or only a few grains of it, the rest being dispersed with great violence, part of it flying against the faces of persons who assisted in making the experiments. This circumstance, together with the charcoal being a conductor of electricity, makes it so extremely difficult to fire gunpowder by electrical explosions; and it is evidently owing to this lateral force, that parts of the melted wire fly so many ways, and to so great a distance from the place of explosion.

This lateral force is exerted not only in the neighbourhood of an explosion, when it is made between pieces of metal in the open air, but also when it is transmitted through wires that are not thick enough to conduct it perfectly; and the smaller the wire, and the more complete the fusion, the greater is the dispersion of light bodies placed near it. At one time, when the wire was not melted, but turned blue by the explosion (in which case it generally assumes a dusky red, which lasts but for a moment),
there

there was a small dispersion from every part of the wire, but by no means so great as it would have been if it had been melted, or only heated to a greater degree.

By a considerable number of trials I found, that a greater force of explosion would move light bodies at a greater distance ; but the smaller the bodies were, the less was this difference ; so that I supposed, that if they had no weight at all, they would, probably, be moved at the same distance by the explosion from any quantity of coated surface, charged equally high ; but there was a great difference in the weights removed by different forces at the same distance. Placing the same piece of cork at the same distance from the place of explosion, I found that the discharge of one jar removed it $\frac{1}{4}$ th of an inch, two jars $1\frac{1}{4}$ th, three $1\frac{3}{4}$ ths, and four about two inches ; so that I do not wonder at very heavy bodies being moved from their places, and to considerable distances, by strong flashes of lightning.

That the immediate cause of this dispersion of bodies in the neighbourhood of electrical explosions is not their being suddenly charged with a quantity of electric matter, and therefore flying from others that are equally charged with it, is, I think, evident from the following experiments and observations. I never observed the least sensible attraction of these light bodies to the brass rods, through which the explosion passed, or to the electric matter passing between them, previous to this repulsion, though I used several methods which could not have failed to shew it, if there had been any such thing. Sometimes I suspended them in fine silken strings, and observed

that they had contracted no electricity after they had been agitated in the manner described above. Sometimes I dipped them in turpentine, and observed that no part of it was found sticking either to the brass rods themselves, or to any part of the table betwixt them and the place where the light bodies had been laid. I even found that the explosion of a battery made ever so near to a brass rod did not so much as disturb the equilibrium of the electric fluid in the body itself: for when I had insulated the rod, and hung a pair of pith balls on the end opposite to that near which the explosion passed, I found that the balls were not in the least moved at the time of explosion, which they would have been, if part of the electric fluid, natural to the body, had been driven, though but for a moment, towards the opposite end. I also observed, that the effect was the same, when the explosion was made to pass through one of the knobs of the insulated rod. This lateral force was evident through thin substances of various kinds interposed between the explosion and the bodies removed by it, as paper, tin-foil, and even glass; for when some grains of gunpowder were put into a thin phial, close stopped, and held near the explosion of a battery, they were thrown into manifest agitation.

I therefore think it most probable, that this lateral force is produced by the expulsion of the air from the place where the explosion is made. For the electric matter makes a *vacuum* of air in its passage; and, this air, being displaced suddenly, gives a concussion to all the bodies that happen to be near it. Hence the removal of the light bodies, and the agitation
commu-

communicated to the thin substances, and to the air, and the light bodies placed beyond them.

The only objection to this hypothesis is, that this lateral force is not so much less *in vacuo* as might be expected, when the air is supposed to receive the concussion first, and to communicate it to other bodies ; but it must be considered, that the most perfect *vacuum* we can make with a pump is not free from air. I have tried to make this experiment in a Torricellian *vacuum*, but could not succeed at that time. Besides, as the electric matter, of which an explosion consists, must take a wider path *in vacuo*, if not equally fill the whole space, it may affect a body in its passage, without the intervention of any air. In condensed air, this lateral force was not, as far as I could perceive, much increased.

Willing to feel what kind of an impulse it was that acted upon bodies, when they were driven away by this lateral force of electricity ; I held my finger near the path of an explosion of the battery, passing over the surface of a green leaf, when I felt a stroke, as of something pushing against my finger. Several corks, placed in the same situation, were driven to a considerable distance by the same explosion.

Recollecting that this power, which I now call the lateral force of electrical explosions, must be the same with that which gives the concussion to water, mentioned in my experiments to imitate an earthquake, and to vegetable and animal substances, over the surface of which it passes ; and being determined to make a more satisfactory trial of it than I had ventured to do before, I laid a green leaf upon the palm of my hand, intending to make the explosion pass over the leaf ;

leaf; but the leaf was burst, and torn to pieces, and the explosion, passing over my hand, gave it a violent jar, the effect of which remained, in a kind of tingling, for some time.

Lastly, in order to judge the most perfectly of this force, I laid a chain communicating with the outside of the battery upon my bare arm, above the wrist, and bringing the discharging rod near the flesh, within about two inches and a half of the chain, I made the explosion pass over that quantity of the surface of the skin. Had I taken a greater distance, I was aware that the explosion would have entered the flesh; which, I was sensible, would have given a painful convulsion to the muscles through which it passed. In this case the sensible effect was very different from that, being the same external concussion as before; and I have sometimes thought, that the sensation is not disagreeable. However, the hairs upon the skin were singed, and curled up along the whole path of the explosion, and for the space of about half an inch on each side of it: also the *papillæ pyramidales* of the skin were raised, as when a person is shivering with cold. This was also the case in every part of the arm which the chain touched, and even that part of it which was not in the circuit. Both the path of the explosion, and the place on which the chain had lain, had a redness which remained till the next day. Sometimes the flesh has contracted a blackness by this experiment, which has remained for a few hours.

*X. Various Experiments on the Force of
Electrical Explosions. By Joseph Priestley,
LL. D. F. R. S.*

Read March 2,
1769. **M**AKING the explosion of a battery pass over the surface of a green cabbage-leaf, I observed that it left a track near $\frac{1}{4}$ th of an inch in breadth, exceedingly well defined, and distinguishable by a difference of colour from the rest of the leaf. Along this path, also, the firmness of texture in the leaf was entirely destroyed, that part becoming quite flexible, like a piece of cloth. Presently after, it turned yellow, grew withered, and became perfectly brittle.

Willing to try the effect of this explosion passing along the surface of other substances, I laid a piece of common window-glass on the path, pressed by a weight of six ounces; but it was shattered to pieces, and totally dispersed, together with the leaf on which it lay. Placing the black side of a piece of cork-wood upon it, pressed by a weight of half a pound, the leaf was not rent, but the cork was furrowed all the way, a trench being made in it about half an inch in breadth, and a quarter of an inch in depth. Laying the smooth cut surface of the piece of cork, it was furrowed all the way, as if it had been cut with a file, but not near so deep as before. Many of the small pieces, which had been rubbed off in the explosion,

plosion, remained in the furrow. Also the substance of the cork seemed to be shattered, and it was easily rubbed off, a little way into it.

I made this explosion on the surface of some red wine, in a small dish, and kept a part of the same quantity exposed in a similar manner; but I could perceive no difference between them after several days.

The track of an electrical explosion on the surface of the cabbage-leaf, being so well defined, suggested an experiment to ascertain whether there was any sensible momentum in the electric fluid, when it is rushing with violence from one side of a battery to the other. For this purpose I made the explosion pass over the leaves when they were cut in right and acute angles; so that the shortest path, from the inside to the outside of the battery, was to turn close at the angle; and observed that it was not diverted from its course, in the least degree, by the rapidity of its own motion, but that it had turned exactly at the angle, and kept as close to the opposite side, as if the motion had begun at the angle. The electric matter had, however, been evidently attracted by the veins of the cabbage-leaf, having pursued them a little way, at least having sensibly affected them, wherever it met with them in its passage.

This experiment suggested another, intended to determine whether the force of an explosion was at all diminished by being diverted from a right-lined course, and made to turn in a great number of angles. To do this, I first found, by a great number of trials, what length of a small iron wire I was able to melt with a battery of about twenty square feet, in the middle

middle of a circuit of about three yards of brass wire, considerably thicker than the iron, and stretched in two right lines, suspended on silken strings. The length of the iron wire, melted in these circumstances, was about three inches. I then took the same brass wire, and fixing pins into a board of baked wood, twisted it about them, making it turn in a very great number of acute angles, and I put three inches of the same iron wire in the middle of this crooked circuit, that I had done in the straight one; so that the electric matter in the explosion was obliged to make a great number of turns at acute angles, before it could come to the iron wire; but I always found that the same length of iron wire was melted in these circumstances as in the other, and not the least difference was perceived in the force.

But though the form of the wire through which an explosion passed, made no difference in its force, I found a very remarkable difference occasioned by the length of the circuit in wires of the same thickness; and which, I own, surprized me very much.

In order to ascertain the practicability of firing mines by electrical explosions, I took twenty two yards of small brass wire (but so thick, however, that I could not have melted the least part of it by the force of any battery I have ever constructed), and extending it along a dry boarded floor, with a small piece of iron wire, and a cartridge of gunpowder about it, in the place that was most remote from the battery, I found that, upon the discharge, the wire was not melted, nor the gunpowder exploded; also the report was very faint. In other circumstances, a charge of the same battery was able to melt more than

nine inches of this iron wire; and this same cartridge was easily fired near the battery, connected with shorter pieces of the same brass wire; so that the diminution of force must have been owing to the length of the circuit.

In the place of this small brass wire, I substituted an iron wire one fifth of an inch thick, when about half an inch of the small iron wire was exploded; so that the force was not lessened so much in a circuit of the thick iron wire, as it had been in one of the small brass wires. In order to judge how much of the force might be lost by nearer circuits, consisting of less perfect conductors, I joined the middle of the circuit made by the iron wire with water, in which both the wires were immersed. The effect was, that the small iron wire was only made red-hot, but not exploded as before.

Being sensible how much depended upon avoiding lesser circuits, whereby part of the fire of an explosion might return to the battery, without reaching the extremity of the circuit, where I intended the whole of its force to be exerted, in the remaining experiments, I insulated half the circuit of iron wire. There was no occasion for insulating the whole circuit; for if there was but one passage to, or from the middle of it, there could be but one from, or to it. In this method it was easy to ascertain what loss of force was occasioned by the length of the circuit, as every other circumstance was carefully excluded; and it presently appeared to be very considerable; for tho' I could melt nine inches of the small iron wire at the distance of fifteen yards from the battery; when I tried twenty yards, I found that I was just able to make
fix

six inches of it red-hot. The battery in these experiments was in the house, and the wires of which the circuit consisted were conveyed by filken strings into a garden adjoining to the house.

Mentioning this loss of force occasioned by the length of the circuit in electrical explosions to Dr. Franklin, he told me that the same observations had occurred to him, and that he had also been disappointed in an attempt to fire gunpowder at a distance from his battery.

Struck with this appearance, I endeavoured to ascertain the quantity of this obstruction, by trying what other courses the electric fire would chuse preferably to a long metallic circuit. In the first place, taking about a yard of the small brass wire, mentioned above, I disposed it in the manner described below, connecting one of the ends with the outside of the battery, and the other with the inside. In the first place, I brought the parts *a* and *b* (near the two extremities) into contact, and, upon the discharge, found there had been a fusion in that place, and that a great part of the fire had taken the shorter circuit, though it had been obliged to quit the wire in one place, and enter it again in another. Afterwards I removed the parts *a* and *b* to a small distance from one another, and, upon the explosion, observed a strong spark pass between them. Removing them to greater and greater distances, I found the explosion to pass above one third of an inch in the air, rather than make the circuit of the continued wire. Using a longer and smaller iron wire, the passage through the air exceeded half an inch. I then took four or five yards of iron wire one tenth of an inch thick, when

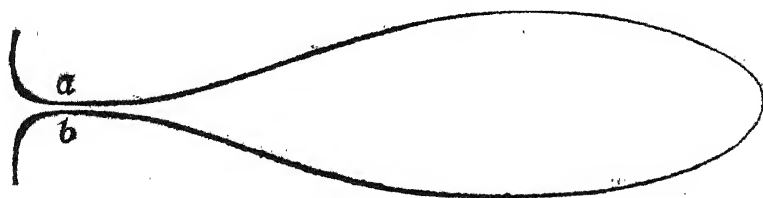
the passage through the air was still half an inch; and taking three yards and a half of wire that was one fifth of an inch thick, the spark in the air was half an inch, and sometimes near three quarters of an inch. Making use of only half the length of this wire, the passage through the air was only half that distance, or $\frac{1}{4}$ th of an inch. When I kept the place of near contact about the middle of this wire, and made the explosion at the extremities of the whole wire, I was obliged to bring them about as near again, *i. e.* to little more than one eighth of an inch, before the passage would be through the air; so that the force of the whole explosion must have been greatly weakened by its passing through so much of the wire. Lastly, I took a pair of kitchen tongs, the legs of which were two feet, and the smallest part of them above half an inch in diameter; when the circuit was made about one sixth of an inch in the air (for at that distance from one another the ends of the tongs had been fixed) rather than through four feet of that thick iron.

Notwithstanding this evident passage of the electric matter through the air, at the same time that a metallic circuit was provided for it; it was certain that the whole of the charge did not pass in the air: for when I extended $\frac{1}{3}$ d. of an inch of small iron wire between *a* and *b*, it was only made red-hot by the discharge; whereas above two inches of it would have been exploded, if there had been no other metallic circuit at all.

As the electric fire meets with so much obstruction in passing through a circuit of iron of this thickness, I make no doubt but that it is considerably obstructed

in

in passing through metallic circuits of any thickness whatever ; and that it would prefer a very short passage thro' the air, if they were made even of no great length. In this method the different degrees of conducting power in different metals may be tried, using metallic circuits of the same length and thickness, and observing the difference of the passage through the air in each. N. B. A common jar answers as well, in these experiments, as a large battery. It is evident, from many experiments, that the whole fire of an explosion does not pass in the shortest and best circuit ; but that, if inferior circuits be open, part will pass in them at the same time. Of this I made the following satisfactory trial. I took an iron chain, and laid it upon a table, in contact with a charged jar ; so that the parts of it made two circuits for the discharge, which I could vary at pleasure ; and I observed that, when one of the circuits was but half an inch, and the other more than half a yard ; yet, if the charge was high, it always went in them both, there being considerable flashes between the links of the remotest part of the chain. If the charge was weak, it passed in the shortest circuit only.



It is evident, that when the wires of a battery are not in close contact, there must be some loss of force

in the discharge ; but this never appeared to me to be very considerable. In order to ascertain it by experiment, I first found, by repeated trials, what length of a piece of iron wire I was able to melt with a battery consisting of twenty jars, with the wires and connecting rods quite loose, and a chain to join the rods belonging to each row of jars, which is the manner in which I have generally constructed them. In these circumstances, I found the battery was able to melt something more than two inches and a half of the wire. I then soldered the wires of each jar to the rod which connected them, and also soldered another rod to all these, instead of the chain which I had used ; so that I avoided near a hundred sparks in the discharge, at each of which there must have been some loss of force ; but I did not find, after many trials, that the strength of the battery had been thereby sensibly diminished : for I could not melt three inches of the same piece of wire in these circumstances. It was only made red-hot, which is equivalent to the melting and exploding of little more than two inches and a half.

XI. *Abstract of a Letter from Stephen De Visme, Esq; at Canton, in China, to Henry Baker, F. R. S. containing an Account of an Earthquake at Macao, and a short Description of a singular Species of Monkeys without Tails, found in the interior Part of Bengal. Communicated by Mr. Baker.*

Canton, January 7, 1768.

DEAR SIR,

Read March 9, 1769. **T**HE following account of an earthquake, at Macao, was sent me by a friend at that place, in a letter, dated November 23, 1767. His words are: “ Last night, at 50
 “ minutes after nine o’clock, we were all surprized
 “ with a heavy shock of an earthquake, which continued above a minute. This shock was so great
 “ that the house rocked, and I was afraid we were all
 “ going down into the bowels of the earth. Another
 “ shock we felt five minutes after eleven o’clock, but
 “ not so great: and at three this morning another
 “ pretty great. In all we have had five shocks, but
 “ the first the greatest. It came with a rolling, and
 “ a dreadful noise in the air; so that at first some
 “ people

“ people thought it to be the firing of guns, or thunder at some distance. At the first shock I could hardly hold my feet; but, thank God, no bad accident has happened; and I hope the Almighty will deliver us from any more of these frightful shocks. I was up almost the whole night. The wind was northerly, but faint, and it was sultry hot; the sky close and cloudy, and not a star to be seen. The oldest people here say, they never remember to have felt so violent a shock, and of so long continuance. The ships in the harbour shook and whirled about, and those on board imagined at first that it had been a whirlwind.”——

At Manilla earthquakes are often very violent, so as to overturn steeples, houses, and other buildings; and I observed, when I was there, that, to prevent such accidents, their timbers in building are placed in a very particular manner; they have no Attic story, only ware-houses, and one floor over them.

Perhaps the drawing, which I now send you, of a singular sort of Monkies, male and female, may not prove unacceptable. These animals are called Golok, or wild people, and are thought to be originally a mixture with the human kind, having no tails. They come out of the forests in the interior part of Bengal, from the country called Mevat. They inhabit the woods: their food is fruit, leaves, bark of trees, and milk: flesh only when caught. They are very gentle, and extremely modest. They are of the height of a man; their teeth are as white as pearls; their legs and arms are in due proportion to their body, which is very genteel. Some of them
were



were brought to Decca; and what I now send you is a copy of the original drawing *. See TAB. III.

I remain, dear Sir,

Your affectionate

humble servant,

Stephen De Visme.

* The monkey, of which Mr. De Visme has sent this drawing and short account, seems to be very like, if not the same with the ape without a tail, described by Mr. De Buffon in the XIVth volume of the *Histoire Naturelle*, p. 92, under the name of the Gibbon, which it bears in some parts of the East-Indies. This species is found, he says, along the coast of Coromandel, at Malacca, on the Molucca islands, and upon the confines of China. It grows to be upwards of four feet, walks on its hind legs, and sometimes on all four. The hair, with which it is covered, is either brown or black: round about its face is a circle of greyish hairs; its eyes are large, but sunk in its head; its ears naked; its face flat, and of a copper colour. It is of a placid disposition; its motions are gentle; it was fed with bread, fruits, almonds. But the most singular characteristic is, the great length of its arms; and though Mr. De Visme takes no notice of this circumstance in his description, his drawing seems to indicate it; but in a less striking manner than that of Mr. De Buffon, who adds, that, when the animal is upright, it can touch the ground with its hands.

M. M.

XII. *A Letter from Mr. John Robertson, Lib. R. S. to James West, Esq; President of the Royal Society; containing the Demonstration of a Law of Motion, in the Case of a Body deflected by two Forces tending constantly to two fixed Points.*

SIR,

Read April 6, 1769. **T**HE late Mr. Machin (who was, for many years, secretary to the Royal Society, and Gresham professor of astronomy) gave to the editor of the English edition of Sir Isaac Newton's Principia, published in the year 1729, a tract entitled, "The Laws of the Moon's Motion according to Gravity," which was annexed to that impression: Mr. Machin, in the Postscript to that tract, after apologizing for not mentioning the fundamental principles of the demonstration of the propositions relating to the Moon's motions, says, "Some of which, I am apt to think, cannot easily be proved to be either true or false, by any methods which are now in common use."

One of these principles he gives in the following words:

"There is a law of motion which holds in the case where a body is deflected by two forces, tending constantly to two fixed points.

"Which

“ Which is, *That the body, in such a case, will describe, by lines drawn from the two fixed points, equal solids in equal times, about the line joining the said fixed points.*

And (after observing that Sir Isaac Newton has proved, that Kepler’s law of bodies describing equal areas in equal times about the centers of their revolution, cannot hold, whenever the body has a gravity or force to any other than one and the same point) further says, “ there seems to be wanting some such law as I have here laid down, that may serve to explain the motions of the Moon and Satellites, which have a gravity towards two different centers.”

About the year 1742, discoursing with that eminent mathematician, the late William Jones, Esq; F. R. S. on the above-mentioned law, he shewed me its demonstration, and permitted me to take a copy thereof; and as I conceive it to be highly worth preserving, I now offer it to your consideration, about giving it a place in the Philosophical Transactions. I am,

S I R,

Your most humble servant,

February 27,
1769.

J. Robertson.

T A B. IV.

F I G. I.

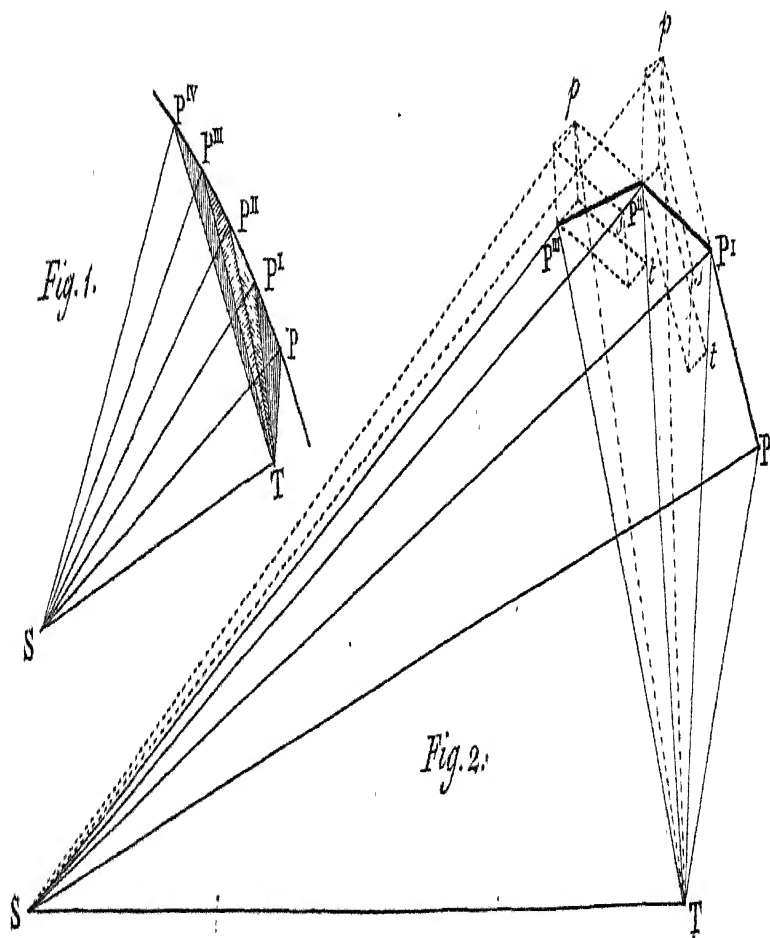
PROPOSITION.

IF a body (P), projected in a given direction, be constantly drawn towards two fixed points (S and T), which are not both in the same plane with the direction, the triangle (SPT), formed by right lines drawn from the body (P) to those fixed points (S and T), shall describe equal solids (STPP', STP'P''), in equal times, about the right line (ST) joining the said points.

FIG. 2. For, suppose a body projected in the direction PP', and acted upon by two centripetal forces towards the fixed points S and T; the angles P'PS, P'PT lying in different planes. Let the time be divided into equal moments.

In the first moment, suppose the body, by its given force, should move along the line PP'; and in the second moment, if no new force was added, it should continue to move in the same right line along P'p = PP'; but when the body has come to P', suppose it acted upon by the two centripetal forces, in the directions P'T, P'S; and let those forces be in proportion to that in the direction PP', as the lines P'λ, P's to the line P'p (= P'P).

With these three right lines P'p, P'λ, P's, complete the parallelopiped P'P''; and the body in P', being acted upon by these three forces, in the directions
P'p,



P^y, P^z, P^s , which forces being as these three lines, shall move along the diagonal of the parallelopiped made by these three lines; so that, in the second moment of time, the body, instead of moving from P to p , shall move from P' to P'' .

Draw the lines SP, SP'' and TP', TP'' , as also Sp, Tp .

Now, the solid $STPP' = \text{solid } STP'p$; for they stand upon equal bases $TP'P, TP'p$, and have one common vertex S , or their common altitude is the perpendicular drawn from S to the plane PTp .

And the solid $STPP'' = \text{solid } STP'p$; for they stand upon the same base STP' , and lie between the same parallel planes pP'', st .

Therefore the solid $STPP' = \text{solid } STPP''$.

In like manner, in the third moment of time, the body at P'' being acted upon by three forces, in the directions $P''P''', P''S, P''T$, shall move along the line $P''P'''$, so as to make the solid $STP''P''' = \text{solid } STPP''$; and so in all succeeding equal moments of time, the triangle formed by right lines drawn from the body to the two fixed points S, T , shall constantly describe little solids, each equal to the solid $STPP'$.

Therefore, the moments of the solids being proportional to the moments of the time in which they are described; the solid itself is proportional to the time in which it is described. Q. E. D.

Some difficulties may, perhaps, seem to arise upon a slight view of only particular cases of this proposition; but, it is conceived, all such must vanish, when the same is thoroughly considered.

For, as in two bodies T and S ; if T is acted upon by S , so as to describe a right line, that is,
if

if T falls directly upon S, no area can then be described by the right line connecting T and S; but yet, this is certainly one of the cases whereby S and T may possibly act upon each other.

So in three bodies, S, T, and P; if P moves in the same plane with S and T, no solid can then be described by the plane whose right lined sides are the lines connecting P to T and S; but yet, this must be one of the cases whereby S, T, and P, may possibly act upon each other.

Received April 13, 1769.

XIII. *A Letter from the Rev. Mr. William Paxton, Rector of Buckland Brewer, in the County of Devon, to Dr. Milles, Dean of Exeter, F. R. S. and Pr. S. A.*

Buckland Brewer, April 7, 1769.

REV. SIR,

Read April 20, 1769. **I** HAD the favour of yours, and with great chearfulness comply with your request, as far as faint words can express what, in reality, is beyond the power of description.

On Thursday, the 2d of March, about four of the clock in the afternoon, a cloud, of a most uncommon blackness, gathered in the west-north-west, and, taking its course to east-south-east, diffused a most prodigious darkness, accompanied with a very copious shower of hail. It passed immediately over the church tower (remarkable for the height both of its situation and structure), and, bursting with incredible fury, poured forth an amazing body of fire, which threw down the south-east pinnacle on the church, and entering (as I suppose) at the breach, shivered a table on which the commandments were written, scorched and discoloured two tomb-stones, broke

broke the windows, and shattered the walls and roof to a great degree. The south-east corner suffered most; where it chiefly forced its way, and tore up the ground on the outside, where it found vent. There is something very extraordinary in the disperſion of the ſtones of the pinnacle to every point of the compaſs, and to different diſtances; ſome of which were 700 pounds weight. I picked up one that weighed almoſt 8 pounds, at the diſtance of 60 perches from the church; and doubt not but others, and perhaps larger ſtones, were carried further: it may be worthy remark alſo, that ſeveral of the ſtones, ſome of which were not ſmall, though they appeared cloſe and firm, yet, on a very ſlight impreſſion of the fingers, mouldered into powder. The exploſion, on the opening of the cloud, was as inſtantaneous as terrible, and equalled the diſcharge of at leaſt a hundred cannon at once.

It is matter of great wonder, that not only the church, but that every houſe in the village, which trembled to its foundation, was not reduced to atoms, or lighted up into a general blaze; and yet, ſtupendous mercy! not a man, woman, child, or beaſt, received the leaſt hurt. I am,

S I R,

Your very obedient,

humble ſervant,

William Paxton.

Received Feb. 23, 1769. — Read April 20, 1769.

XIV. *Abstract from a Meteorological Register kept at the Royal Hospital near Plymouth, during the Year 1768. By W. Farr, M. D. Communicated by W. Watson, M. D. F. R. S.*

N. B. Observations were made twice every day at 9 a. m. and 11 p. m. — The state of the Thermometer, mentioned in the column, is taken from one placed within doors, in an open stair-case, free from the rays of the sun.

1768	Barom. and Thermom.		Rain. Inches.	Weather, with Remarks.
January	3 Highest state of Barom.	30 . 10		January 1st, after a rainy forenoon, wind S. E. the frost set in towards evening, wind N. and continued till the 8th, the lowest state of the thermometer without doors was 20, on the 3d and 4th. During the rest of the month much rain and squally weather, wind for the most part westerly. On the 20th, a violent storm, wind N. N. W. 4. when the Fame man of war was driven from her moorings in Hamoaze.
	1 Lowest state of D°	29 00		
	29 Highest state of Therm.	50		
	3 Lowest state of D°	29	5.755	

68	Barom. and Thermom.		Rain. Inches.	Weather, with Remarks.
				The first week clear and frosty weather, wind N. and moderate; lowest state of thermometer without doors 31; during the remainder of the month constant and heavy rains; from the 7th to the 28th but one fair day; wind, for the most part, W. and S. W. from the 9th to the 13th blew fresh.
ary	5 H. state of Barom.	30	41	
	10 L. state of D°	29	20	
	11 H. state of Thermom.	53		
	3 L. state of D°	39	5.040	
h	19 H. state of Barom.	30	40	
	15 L. state of D°	29	74	
	18 H. state of Thermom.	52		
	3, 6, 7 L. state of D°	39		
				This month proved remarkably dry; although a few drops of rain fell on the 1st, 12th, 15th, and 31st, yet the quantity was too inconsiderable to be collected in the receiver. From the 14th to the 18th the wind was W. the rest of the month E. and N. E. with fine moderate frosty weather.
l	15 H. state of Barom.	30	16	
	29 L. state of D°	29	27	
	16 H. state of Thermom.	56		
	9 L. state of D°	46	3.633	
				Wind continued easterly till the 10th, with dry weather; from that time, to the end of the month, for the most part, westerly, with frequent squalls of wind and rain. 19th W. by N. 3. 28th S. 3.
				Air, the beginning of this month, extremely sharp and cold, with wind fresh at N. E.; remarkable Aurora Borealis in the nights of the 3d, 4th, and

1768.	Barom. and Thermom.		Rain. Inches.	Weather, with Remarks.
May	19 H. state of Barom. 29 L. state of D° 24 H. state of Thermom. 1 L. state of D°	30 29 70 47	15 17 0.815	<p>and 5th; on the 6th the Thermometer, which had stood at 47 and 49 the preceding days, rose, at once, to 60, and the air was very soft and warm; the Aurora Borealis appeared very vivid again on the 24th, with a large and bright halo round the Moon, when the Thermometer rose to 70. Fair the whole month, except the 17th and 29th; wind variable.</p>
June	2 H. state of Barom. 9 L. state of D° 24 H. state of Thermom. often L. state of D°	30 29 64 57	13 24 4.340	<p>From the 6th of this month the rainy season commenced; wind, for the most part, S. and S. W. 24th, a heavy thunder-storm, wind S. E. 28th, stormy weather, with much rain, wind W. 3. Thermometer stood at 57, and below 60, the greatest part of the month.</p>
July	21 H. state of Barom. 7 L. state of D° H. state of Thermom. L. state of D°	30 29 64 61	08 29 4.742	<p>1st, very hot and sultry, with thunder; 2d, 9th, 13th, 15th, 16th, 20th, 30th, fair; the rest rainy. 7th, 12th, and 25th, very tempestuous; wind, during the rest of the month, moderate, and, for the most part, S. W.</p>

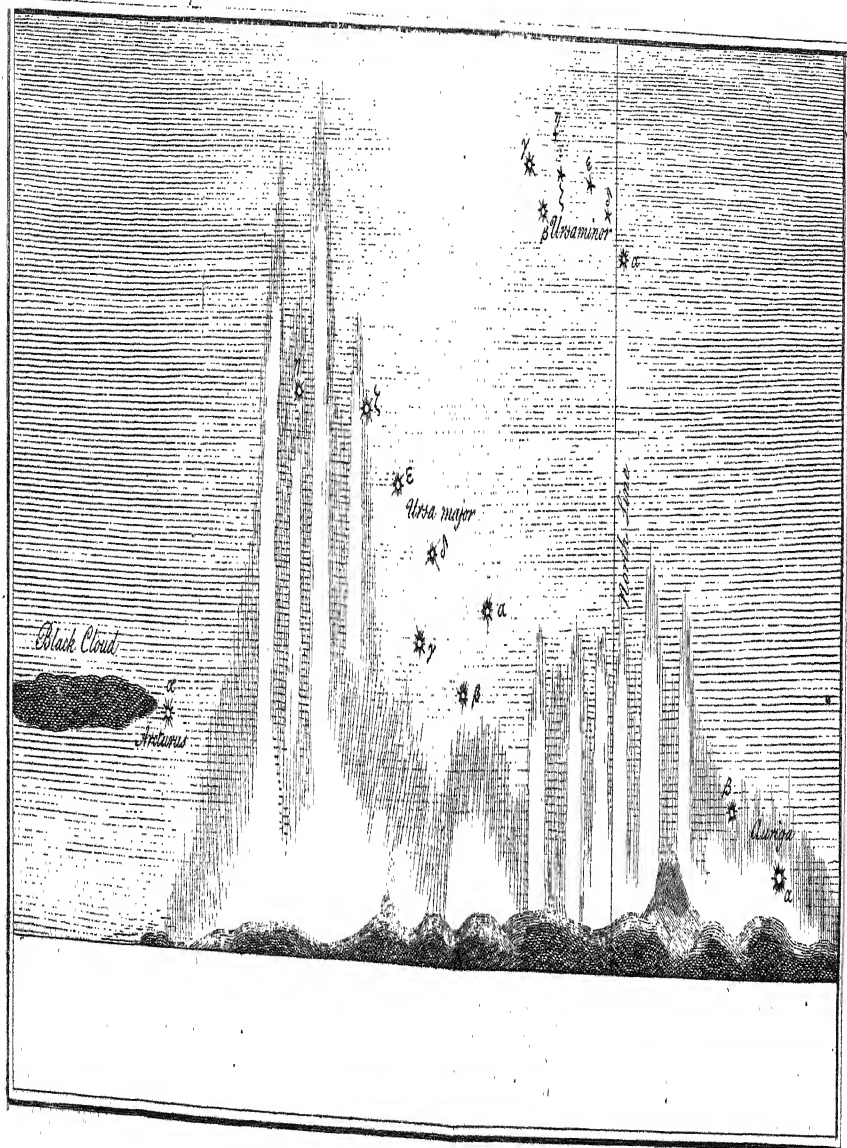
1768.	Barom. and Thermom.		Rain. Inches.	Weather, with Remarks.
August	8 H. state of Barom. 23 L. state of D° frequently H. state of Thermom. 29 L. state of D°	30 08 29 40 67 59	3.005	From the 13th to the 21st, weather hot and sultry; thermometer being, for the most part, at 67, mornings and evenings (middle of the day not observed); from the 16th to the end of the month frequent showers almost every day. Wind variable, but, for the most part, moderate.
Septemb.	14 H. state of Barom. 16 L. state of D° 1, 11, 29 H. state of Thermom. 19 L. state of D°	30 10 29 05 63 54	6.060	From the 1st to the 25th, only four fair days, viz. 3d, 9th, 14th, and 20th, the rest heavy and, often, constant rains, with squally weather, particularly 15th, 16th, 17th, wind W. 3. 22d, E. 3. From 25th to October 3d, fair weather.
October	19 H. state of Barom. 4 L. state of D° 7 H. state of Thermom. 28 L. state of D°	30 10 29 04 62 49	7.350	Although so great a quantity of rain fell this month, yet there were 13 fair days; but the rains the rest of the month were very heavy, particularly, from the 22d to the 31st, near four inches of rain were collected in the receiver. 5th, 6th, 18th, and 30th, stormy. 4th, at 4 p. m. during heavy squalls of rain and wind W. by S. 3. the ϕ fell in the barometer to 29.11; towards evening wind changing to W. by N. it became fair; on the 5th, at 4 p. m. the

1768	Barom. and Thermom.		Rain. Inches.	Weather, with Remarks.
November 5	H. state of Barom.	30	34	the $\frac{8}{16}$ rose to 30.13, or above an inch, in 24 hours; and on the 6th 30.34. On the 22d, at 4 p. m. the $\frac{8}{16}$ fell in the barometer to 28.20, wind N. W. 2'', squally weather from the 21st to the 26th, but much more stormy on the 21st than afterwards. This was the lowest state of the barometer in these parts, since December 12, 1763, when the $\frac{8}{16}$ fell to 27.96. Only six fair days during the month.
22	L. state of D°	28	20	
28	H. state of Thermom.	55		
23	L. state of D°	44	6.490	
December 12	H. state of Barom.	30	31	1st, thunder-storm in the night, wind S. 3. On the 14th, frost set in very sharp, wind E. by N. but the wind changing to S. E. on the 15th, rain succeeded. Thermometer, without doors, not observed. Wind, from the 7th to the 17th, easterly; the rest of the month rain, for the most part, with westerly winds. Eleven fair days this month.
2	L. state of D°	28	94	
23	H. state of Thermom.	83		
14	L. state of D°	34	3.985	
			51.215	Total quantity of rain, during the year 1768.

XV. *An Account of a remarkable Aurora Borealis, observed at the Observatory of the Marine at Paris, by M. Messier, of the Royal Academy of Sciences, and F.R.S. Translated by J. Bevis, M. D. F.R.S.*

Read May 25,
1769.

THE morning of the 6th of August, 1768, was, for the most part, serene, and the afternoon was quite so. At near nine at night, the western horizon was illuminated with a very sensible twilight, which increased greatly upon that which the sun had left. I suspected that this quantity of light could be only owing to a beginning Aurora Borealis; and, accordingly, about ten o'clock, the sky being perfectly clear, excepting one thick cloud, about the same height as Arcturus, represented in my drawing, TAB. V. The Aurora was at that time considerable; several streams of light had then shot up from the horizon. At half an hour after ten, the Aurora occupied nearly one half of the horizon, extending from the west to the north-east, and the horizon seemed to be covered with an uneven thick smoky, from which issued several streamers of light; two of which, to the westward, arose to a great height, passing through the tail of the Great Bear, and were sensibly inclined to each other, tending to unite



in the zenith. Both these luminous streamers kept in a continual agitation, which lasted the whole time of their existence, that is, till eleven o'clock. At the foot of these lights was the furnace, which glowed with rays of light less elevated, and sensibly inclined to the horizon; these were also in continual agitation. At eleven, six streamers, parallel to one another, shot up in the north, under the constellation of the Little Bear; they ascended not so high, but were more conspicuous than the two preceding ones, and their undulations were not so quick. About half an hour after eleven, the sky began to be clouded; at midnight it was so all over, which put an end to all hope of further observing the progress of this phenomenon. In this Aurora I could not discern any lightning or rumbling, as I did in that which appeared in the night of the 21st of May, 1762, described in the fifth and sixth volumes of the *Memoires des Sçavans Etrangers*.

Whilst this phenomenon lasted, the air was calm, and no wind stirring; at least what little there was, was from the north-east. The quicksilver in the barometer stood at 28 inches $1\frac{1}{2}$ line; and Reaumur's thermometer at $20\frac{1}{4}$ degrees. During the whole day the barometer varied only $1\frac{1}{2}$ line, and the thermometer no more than five degrees.

An Account of an Aurora Borealis, observed at Paris, the 5th of December, 1768. By the same.

About seven in the evening, the northern quarter was enlightened with an incipient Aurora Borealis, which increased gradually. At eleven at night it was
very

very conspicuous, numbers of luminous streamers darting up from below the horizon, some of them reaching the zenith ; but none of the streamers lasted any considerable time, and their light was but feeble. Several whitish clouds appeared in the north ; the furnace occupied a great part of the horizon. This phenomenon lasted almost the whole night.

The same Aurora was observed at Berlin, where it lasted from six in the evening till nine. It was also observed at Vienna, where it is said that the needle of a compass lost, during this phenomenon, its usual direction, shifting at first two degrees eastward, and afterwards four degrees the contrary way ; it was at the same time remarked that the electrical machine had acquired an uncommon degree of force.

Received April 20.

XVI. *Observations on the Expectations of Lives, the Increase of Mankind, the Influence of great Towns on Population, and particularly the State of London with respect to Healthfulness and Number of Inhabitants. In a Letter from Mr. Richard Price, F. R. S. to Benjamin Franklin, Esq; LL.D. and F.R.S.*

DEAR SIR,

Read April 27 and
May 4, 1769.

I BEG leave to submit to your perusal the following observations. If you think them of any importance, I shall be obliged to you for communicating them to the Royal Society. You will find that the chief subject of them is the present state of the city of London, with respect to healthfulness and number of inhabitants, as far as it can be collected from the bills of mortality. This is a subject that has been considered by others; but the proper method of calculating from the bills has not, I think, been sufficiently explained.

No competent judgment can be formed of the following observations, without a clear notion of what
VOL. LIX. N the

the writers on *Life Annuities* and *Reversions* have called the *Expectation of Life*. Perhaps this is not in common properly understood; and Mr. De Moivre's manner of expressing himself about it is very liable to be mistaken.

The most obvious sense of the *expectation* of a given life is, "That particular number of years which a life of a given age has an equal chance of enjoying." This is properly the time that a person may reasonably *expect* to live; for the chances *against* his living longer are greater than those *for* it; and, therefore, he cannot entertain an *expectation* of living longer, consistently with probability. This period does not coincide with what the writers on Annuities call the *expectation of life*, except on the supposition of an uniform decrease in the probabilities of life, as Mr. Simpson has observed in his *Select Exercises*, p. 273.—It is necessary to add, that, even on this supposition, it does not coincide with what is called the *expectation of life* in any case of joint lives. Thus, two joint lives of 40 have an even chance, according to Mr. De Moivre's hypothesis*, of conti-

* Mr. De Moivre's hypothesis, here referred to, supposes (as is well known to those who have studied the subject of Life Annuities) an equal decrement of human life through all its stages. That is, it supposes that out of any given number alive at a given age, the same number will die every year till they are all dead. Thus; 86 Mr. De Moivre makes the utmost probable extent of life. The number of years which any given life wants of 86 he calls the *complement* of that life.—56, therefore, is the *complement* of 30; and supposing 56 persons alive at this age, *one* will die every year till, in 56 years, they will be all dead. The like will happen to 46 at 40, to 36 at 50, and so on, for all other ages. This is an

ning

ning together only $13\frac{1}{2}$ years. But the *expectation* of two equal joint lives being (according to the same hypothesis) always a *third* of the *common complement*, it is in this case $15\frac{1}{3}$ years. It is necessary, therefore, to observe, that there is another sense of this phrase which ought to be carefully distinguished from that now mentioned. It may signify “The *mean continuance* of any given *single, joint, or surviving* lives, “ according to any given table of observations:” that is, the number of years which, taking them one with another, they actually enjoy, and may be considered as sure of enjoying, those who live or survive *beyond* that period, enjoying as much *more* time in proportion to their number, as those who *fall short* of it enjoy *less*. Thus, Supposing 46 persons alive, all 40 years of age, and that, according to Mr. De Moivre’s *hypothesis*, one will die every year till they are all dead in 46 years, half 46 or 23 will be their *expectation of life*: that is; The number of years enjoyed by them all will be just the same as if every one of them had lived 23 years, and then died; so that, supposing no interest of money, there would be no difference in value between annuities payable for life to every single person in such a set, and equal annuities payable to another equal set of persons of the same common age, supposed to be all sure of living just 23 years and no more.

excellent *hypothesis*. It eases exceedingly the labour of calculating the values of lives. It is remarkably agreeable to Dr. Halley’s Table of Observations; and, as far as it implies an equal decrement of life, is, in a great measure, confirmed by other Tables.

In like manner; the *third* of 46 years, or 15 years and 4 months, is the *expectation* of two joint lives both 40; and this is also the *expectation* of the survivor. That is; supposing a set of marriages between persons all 40, they will, one with another, last just this time, and the survivors will last the same time; and annuities payable during the continuance of such marriages would, supposing no interest of money, be of exactly the same value with annuities to begin at the extinction of such marriages, and to be paid, during life, to the survivors. In adding together the years which any great number of such marriages and their survivorships have lasted, the sums would be found to be equal.

One is naturally led to understand the *expectation* of life in the first of the senses now explained, when, by Mr. Simpson and Mr. De Moivre, it is called, *the number of years which, upon an equality of chance, a person may expect to enjoy*; or, *the time which a person of a given age may justly expect to continue in being*; and, in the last sense, when it is called, *the share of life due to a person* *. But, as in reality it is always used in the last of these senses, the former language should not be applied to it: and it is in this last sense that it coincides with the *sums* of the *present* probabilities that any given single or joint lives shall attain to the end of the 1st, 2d, 3d, &c. *moments* from this time to the end of their possible existence; or, in the case of survivorships, with the sum of the probabilities that

* See Mr. De Moivre on *Annuities*, p. 65, &c. 4th edition, and Mr. Simpson's *Select Exercises*, p. 255, 273.

there

there shall be a survivor at the end of the 1st, 2d, 3d, &c. *moments*, from this time to the end of the possible existence of survivorship. This coincidence every one conversant in these subjects must see, upon reflecting, that both these senses give the true present value of a life-annuity secured by land, without interest of money*.

* The *sum* of the probabilities that any given lives will attain to the end of the 1st, 2d, 3d, &c. *years* from the present time to the utmost extremity of life (for instance, $\frac{4}{4} + \frac{3}{4} + \frac{2}{4} + \frac{1}{4}$, &c. to $\frac{1}{40} = 22\frac{1}{2}$ for lives of 40, by the *hypothesis*) may be called their *expectation*, or the number of payments due to them, as *yearly annuitants*. The sum of the probabilities that they will attain to the end of the 1st, 2d, 3d, &c. *half years* (or, in the particular case specified, $\frac{9}{9} + \frac{8}{9} + \frac{7}{9} + \frac{6}{9} + \frac{5}{9}$, &c. $= 9\frac{1}{2}$ *half years*, or $22\frac{3}{4}$ *years*) is their expectation as *half yearly annuitants*. And the sums just mentioned of the probabilities of their attaining to the end of the 1st, 2d, 3d, &c. *moments* (equal in the same particular case to 23 years) is properly their *expectation of life*, or their *expectation* as annuitants secured by land.

Mr. De Moivre has concealed the demonstrations of the rules he has given for finding these *expectations* of life, and only intimated, in general, that he discovered them by a calculation deduced from the method of fluxions, p. 66, of his *Treatise on Annuities*. It will, perhaps, be agreeable to some to see how easily they are deduced in this method upon the hypothesis of an equal decrement of life.

Let x stand for a moment of time and n the complement of any assigned life. Then $\frac{n-x}{n}$, $\frac{n-2x}{n}$, $\frac{n-3x}{n}$, &c. will be the *present* probabilities of its continuing to the end of the 1st, 2d, 3d, &c. moments; and $\frac{n-x}{n}$ the probability of its continuing to the end of x time. $\frac{n-x}{n} \times x$ will therefore be the *fluxion* of the sum of the probabilities, or of an *area* representing this sum, whose

This

This period in *joint* lives, I have observed, is *never*

ordinates are $\frac{n-x}{n}$, and *axis* x .—The *fluent* of this expression,

or $x - \frac{x^2}{2n}$ is the sum itself for the time x ; and this, when $x=n$, becomes $\frac{1}{2}n$, and gives the *expectation* of the assigned life, or the sum of all the probabilities just mentioned for its whole possible

duration.—In like manner: Since $\frac{n-x^2}{n^2}$ is the probability that

two equal joint lives will continue x time $\frac{n-x}{n^2} \times x$ will be the *fluxion* of the sum of the probabilities. The *fluent* is

$x - \frac{x^2}{n} + \frac{x^3}{3n^2}$, which when $n=x$ is $\frac{2}{3}n$ the *expectation* of two

equal joint lives.—Again: Since $\frac{n-x}{n} \times \frac{2x}{n}$ is the probability that there will be a survivor of two equal joint lives at the

end of x time, $\frac{n-x}{n} \times \frac{2x}{n} \times x$ will be the *fluxion* of the sum of

the probabilities; and the *fluent*, or $\frac{x^2}{n} - \frac{2x^3}{3n^2}$ is (when $x=n$) $\frac{1}{3}n$, or the *expectation* of survivorship between two equal lives,

which therefore appears to be equal to the *expectation* of their joint continuance. The *expectation* of two *unequal* joint lives

found in the same way is $\frac{m}{2} - \frac{m^2}{6n}$, m being the *complement* of the oldest life, and n the *complement* of the youngest. The whole

expectation of survivorship is $\frac{n}{2} - \frac{m}{2} + \frac{m^2}{3n}$. The *expectation*

of survivorship on the part of the oldest is, $\frac{m^2}{6n}$; and the ex-

pectation on the part of the youngest is, $\frac{n}{2} - \frac{m}{2} + \frac{m^2}{6n}$. It is

easy to apply this investigation to any number of joint lives, and to all cases of survivorship.

I have above endeavoured to shew distinctly how the *expectations* of *single* lives may be found, agreeably to any Table of Observations, without having recourse to any principles, except such as are plain and common.

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the same with the period which they have an equal chance of enjoying ; and in single lives, I have observed, they are the same only on the supposition of an uniform decrease in the probabilities of life. If this decrease, instead of being always uniform, is *accelerated* in the last stages of life, the former period, in single lives, will be *less* than the latter ; if *retarded*, it will be *greater*. -

It is necessary to add, that the number expressing the former period, multiplied by the number of single or joint lives whose expectation it is added annually to a society or town, gives the whole number living together, to which such an annual addition would in time grow. Thus ; since 19, or the third of 57, is the *expectation* of two joint lives whose common age is 29, or common *complement* 57, twenty marriages every year between persons of this age would, in 57 years, grow to 20 times 19, or 380 marriages always existing together. The number of *survivors* also arising from these marriages, and always living together, would, in twice 57 years, increase to the same number. And, since the *expectation* of a single life is always half its *complement*, in 57 years likewise 20 single persons aged 29, added annually to a town, would increase to 20 times 28.5 or 570 ; and when arrived at this number, the deaths every year will just equal the accessions, and no further increase be possible.

It appears from hence, that the particular proportion that becomes extinct every year, out of the whole number constantly existing together of single or joint lives, must, wherever this number undergoes no variation,

variation, be exactly the same with the *expectation* of those lives at the time when their existence commenced. Thus; was it found that a 19th part of all the marriages among any body of men, whose numbers do not vary, are dissolved every year by the deaths of either the husband or wife, it would appear that 19 was, at the time they were contracted, the *expectation* of these marriages. In like manner; was it found in a society, limited to a fixed number of members, that a 28th part dies annually out of the whole number of members, it would appear that 28 was their common expectation of life at the time they entered. So likewise; were it found in any town or district, where the number of births and burials are equal, that a 20th or 30th part of the inhabitants die annually, it would appear that 20 or 30 was the *expectation* of a child just born in that town or district. These *expectations*, therefore, for all *single* lives, are easily found by a *Table of Observations*, shewing the number that die annually at all ages, out of a given number alive at those ages; and the general rule for this purpose is “ to divide
 “ the sum of all the living in the Table at the age
 “ whose expectation is required, and at all greater
 “ ages, by the sum of all that die annually at
 “ that age, and above it; or, which is the same, by
 “ the number in the Table of the living at that age;
 “ and half subtracted from the quotient will be the
 “ required *expectation*.” Thus, in Dr. Halley’s Table, the sum of all the living at 20 and upwards is 20,724. The number living at that age is 598; and the former number divided by the latter, and half

half unity * subtracted from the quotient, gives 34.15 for the *expectation* of 20. The expectation of the same life by Mr. *Simpson's* Table, formed from the bills of mortality of London, is 28.9.

These observations bring me to the principal point which I have had all along in view. They suggest to us an easy method of finding the number of inhabitants in a place from a *Table of Observations*, or the *bills of mortality* for that place, supposing the yearly births and burials equal. “ Find by the “ Table, in the way just described, the *expectation* “ of an infant just born, and this, multiplied by the “ number of yearly births, will be the number of “ inhabitants.” At *Breslaw*, according to Dr. Halley’s Table †, though half die under 16, and therefore an infant just born has an *equal chance* of living only 16 years, yet his *expectation*, found by the rule I have given, is near 28 years; and this, multiplied by 1238 the number born annually, gives 34,664,

* This subtraction is necessary, because the *divisor* ought to be made as much greater than the number dying annually given in the Table, as the *expectation*, with $\frac{1}{2}$ unity added, is greater than the *expectation*, on account of the number that will die, in the course of the year, out of those who are continually added, in order to preserve the number of the living the same.

In other words: If we conceive the *recruit* necessary to supply the *waste* of every year to be made always at the *end* of the year, the *dividend* ought to be the *medium* between the numbers living at the *beginning* and the *end* of the year; that is, it ought to be taken *less* than the sum of the living in the Table at and above the given age, by *half* the number that die in the year; the effect of which *diminution* will be the same with the *subtraction* I have directed.

† Vid. Lowthorp’s Abridgment of the Philosophical Transactions, vol. III. p. 669.

the number of inhabitants. In like manner; it appears from Mr. Simpson's Table, that, though an infant just born in London has not an *equal chance* of living 3 years, his *expectation* is 20 years; and this number, multiplied by the yearly births, would give the number of inhabitants in London, were the births and burials equal. The medium of the yearly births, for the last 10 years, has been 15,710. This number, multiplied by 20, is 314,200; which is the number of inhabitants that there would be in London, according to the bills, were the yearly burials no more than equal to the births: that is, were it to support itself in its number of inhabitants without any supply from the country. But for the last 10 years, the burials have, at an average, been 22,956, and exceeded the christenings 7,246. This is, therefore, at present, the yearly addition of people to London from other parts of the kingdom, by whom it is kept up. Suppose them to be all, one with another, persons who have, when they remove to London, an *expectation* of life equal to 30 years. That is; suppose them to be all of the age of 18 or 20, a supposition certainly far beyond the truth. From hence will arise, according to what has been before observed, an addition of 30 multiplied by 7,246, that is 217,380 inhabitants. This number, added to the former, makes 531,580; and this, I think, at most, would be the number of inhabitants in London were the bills perfect. But it is certain that they give the number of births and burials too little. There are many burying-places that are never brought into the bills. Many also emigrate to the navy and army and country; and these ought to be added to the
number

number of deaths. What the deficiencies arising from hence are, cannot be determined. Suppose them equivalent to 6000 every year in the births, and 6000 in the burials. This would make an addition of 20 times 6000 or 120,000 to the last number, and the whole number of inhabitants would be 651,580. If the burials are deficient only two thirds of this number, or 4000, and the births the whole of it; 20 multiplied by 6000, must be added to 314,290 on account of the defects in the births: and, since the excess of the burials above the births will then be only 5,246; 30 multiplied by 5,246 or 157,380, will be the number to be added on this account; and the sum, or number of inhabitants, will be 591,580. But if, on the contrary, the burials are deficient 6000, and the births only 4000; 80,000 must be added to 314,290, on account of the deficiencies in the births; and 30 multiplied by 9,246, on account of the excess of the burials above the births, and the whole number of inhabitants will be 671,580.

Every supposition in these calculations seems to me too high. *Emigrants* from London are, in particular, allowed the same *expectation* of continuance in London with those who are born in it, or who come to it in the firmest part of life, and never afterwards leave it; whereas it is not credible that the former *expectation* should be so much as half the latter. But I have a further reason for thinking that this calculation gives too high numbers, which has with me irresistible weight. It has been seen that the number of inhabitants comes out less on the supposition, that the defects in the christenings are greater

than those in the burials. Now it seems evident that this is really the case; and, as it is a fact not attended to, I will here endeavour to explain distinctly the reason which proves it.

The proportion of the number of births in London, to the number who live to be 10 years of age, is, by the bills, 16 to 5. Any one may find this to be true, by subtracting the *annual medium* of those who have died under 10, for some years past, from the *annual medium* of births for the same number of years.—Now, tho', without doubt, London is very fatal to children, yet it is incredible that it should be so fatal as this implies. The *bills*, therefore, very probably, give the number of those who die under 10 too great in proportion to the number of births; and there can be no other cause of this, than a greater deficiency in the *births* than in the *burials*. Were the deficiencies in both equal, that is, were the *burials*, in proportion to their number, just as deficient as the *births* are in proportion to *their* number, the proportion of those who reach 10 years of age to the number born would be right in the *bills*, let the deficiencies themselves be ever so considerable. On the contrary, were the deficiencies in the *burials* greater than in the *births*, this proportion would be given too great; and it is only when the former are least that this proportion can be given too little.—Thus; let the number of annual *burials* be 23,000; of *births* 15,700; and the number dying annually under 10, 10,800. Then 4,900 will reach 10 of 15,700 born annually; that is, 5 out of 16.—Were there no deficiencies in the *burials*, and were it fact that only *half* die under 10, it would follow, that there was an
annual

annual deficiency equal to 4,900 subtracted from 10,800, or 5,900 in the *births*.—Were the *births* a third part too little, and the *burials* also a third part too little, the true number of *births*, *burials*, and of *children dying under 10*, would be 20,933—30,666, and 14,400; and, therefore, the number that would live to 10 years of age would be 6,533 out of 20,933, or 5 of 16 as before.—Were the *births* a third part, and the *burials* so much as two-fifths wrong, the number of *births*, *burials*, and children dying under 10 would be 20,933—32,200 and 15,120; and, therefore, the number that would live to 10 would be 5,813 out of 20,933, or 5 out of 18.—Were the *births* a 3d part wrong, and the *burials* but a 6th, the foregoing numbers would be 20,933—26,833—12,600; and, therefore, the number that would live to 10 would be 8,333 out of 20,933, or 5 out of 12.56: and this proportion seems as low as is consistent with any degree of probability. It is somewhat less than the proportion in Mr. Simpson's Table of *London Observations*, and near *one half* less than the proportion in the Table of *Observations* for Breslaw, where it appears that above 9 of 16 live to be 10, and that *one half* live to be 16. The deficiencies, therefore, in the *births* cannot be much less than double those in the *burials**; and the least numbers I have given

* One obvious reason of this fact is, that *none* of the *births* among *Jews*, *Quakers*, *Papists*, and the *three denominations of Dissenters* are included in the bills, whereas *many* of their *burials* are. It is further to be attended to, that the abortive and still-born, amounting to about 600 annually, are included in the burials, but never in the births. If we add these to the christen-

must,

must, probably, be nearest to the true number of inhabitants. However, should any one, after all, think that it is not improbable that only 5 of 16 should live in London to be 10 years of age, or that above *two thirds* die under this age, the consequence of admitting this will still be, that the foregoing calculation has been carried too high. For it will from hence follow, that the *expectation* of a child just born in London cannot be so much as I have taken it. This *expectation* is 20, on the supposition that half die under 3 years of age, and that 5 of 16 live to be 29 years of age, agreeably to Mr. Simpson's Table. But if it is indeed true, that *half* die under 2 years of age, and 5 of 16 under 10, agreeably to the *bills*, this expectation must be less than 20, and all the numbers before given will be considerably reduced.

Upon the whole: I am forced to conclude from these observations, that the second number I have given, or 651,580, though short of the number of inhabitants commonly supposed in London, is, very probably, *greater*, but cannot be much *less*, than the true number. Indeed, it is in general evident, that in cases of this kind numbers are very much over-rated. The ingenious Dr. Brakenridge *, 14 years ago, when the bills were lower than they are now, from the number of houses, and allowing six to a house, made the number of inhabitants 751,800. But his method of determining the num-

ings, preserving the burials the same, the proportion of the born, according to the bills, who have reached ten for the last sixteen years, will be very nearly one *third* instead of *five sixteenths*.

* Vid. Phil. Transact. vol. XLVIII.

ber of houses is too precarious ; and, besides, six to a house is, probably *, too large an allowance. Many families now have two houses to live in. The magistrates of Norwich, in 1752, took an exact account of both the number of houses and individuals in that city. † The number of houses was 7,139, and of

* If this is true, Dr. Brakenridge has also over-rated the number of people in England. The number of houses rated to the window tax he had, he says, been certainly informed was 690,000. The number of cottages not rated was not, he adds, accurately known ; but from the accounts given in it appeared, that they could not amount to above 200,000 ; and, allowing 6 to a house, this would make the number of people in England 5,340,000. But if 5 to a house should be a juster allowance, the number will be 4,450,000. The number of people in Scotland he reckons 1,500,000, and in Ireland 1,000,000.—See a Letter to George Lewis Scott, Esq; Phil. Transact. vol. XLIX: p. 877. 1756.

† Vid. Gentleman's Magazine for 1752, and Dr. Short's *Comparative history of the increase of mankind*, p. 38. In page 58 of this last work the author says, that, in order to be fully satisfied about the number of persons to be allowed to a family, he procured the true number of families and individuals in 14 market towns, some of them considerable for trade and populousness ; and that in them were 20,371 families, and 97,611 individuals, or but little more than $4\frac{3}{4}$ to a family. He adds, that, in order to find the difference in this respect between towns of trade and country parishes, he procured from divers parts of the kingdom the exact number of families and individuals in 65 country parishes. The number of families was 17,208 ; individuals 76,284 ; or not quite $4\frac{1}{2}$ to a family.—In the place I have just referred to, in the Gentleman's Magazine, there is an account of the number of houses and inhabitants in Oxford exclusive of the colleges, and in Wolverhampton, Coventry, and Birmingham, for 1750. The number of persons to a house was, by this account, $4\frac{4}{5}$ in the two former towns, and $5\frac{3}{4}$ in the two latter. It seems, therefore, to appear that 5 persons to a house is an allowance large enough for London, and too large for England in general:

indivi-

individuals 36,169, which gives nearly 5 to a house. — Another method which Dr. Brakenridge took to determine the number of inhabitants in London was from the annual number of burials, adding 2000 to the bills for omissions, and supposing a 30th part to die every year. In order to prove this to be a moderate supposition he observes that, according to Dr. Halley's Observations, a 34th part die every year at Breſlaw. But this observation was made too inadvertently. The number of annual burials there, according to Dr. Halley's account, was 1174, and the number of inhabitants, as deduced by him from his Table, was 34,000, and therefore a 29th part died every year. Besides; any one may find, that in reality the Table is constructed on the supposition, that the whole number born, or 1238, die every year; from whence it will follow that a 28th part died every year. * Dr. Brakenridge, therefore, had he attended to this, would have stated a 24th part as the proportion that dies in London every year, and this would have taken off 150,000 from the number he has given. But even this must be less than the just proportion. For let three fourths of all who either die in London or migrate from it, be such as have been born in London; and let the rest be persons who have removed to London from the country or from foreign nations.

* Care should be taken, in considering Dr. Halley's Table, not to take the first number in it, or 1000, for so many just born. 1238, he tells us, was the annual medium of births, and 1000 is the number he supposes all living at one year and under. It was inattention to this that led Dr. Brakenridge to his mistake.

The *expectation* of the former, it has been shewn, cannot exceed 20 years, and 30 years have been allowed to the latter. One with another, then, they will have an *expectation* of $22\frac{1}{2}$ years. That is, one of $22\frac{1}{2}$ will die every year. * And, consequently,

* The whole number of inhabitants in Rome, in the year 1761, was 157,452; of whom 90,239 were males, and 67,213 females. And the annual medium of births, for 3 years from 1759 to 1761, was 5,167, and of burials 7,153. According to this account, therefore, a 22d part of the inhabitants die in Rome every year. See Dr. Short's *Comparative History of the increase and decrease of mankind in England and several countries abroad*, p. 59, 60.—In Berlin, as the same author relates, p. 69, in six years, from 1734 to 1740, the annual medium of births was 3,504, of burials 3,639, and the number of inhabitants was 68,197; males 32,990, and females 35,207. A 19th part, therefore, of the inhabitants of Berlin are buried every year. As numbers taken by actual survey are generally too little, suppose, in the present instance, an error committed in reckoning the number of inhabitants, equal to a 10th of the whole number, or to the whole number of children under 5; and suppose likewise no omissions in the burials. The consequence will be, that about 1 in 21 are buried at Berlin every year.—At Dublin, in the year 1695, the number of inhabitants was found, by an exact survey, to be 40,508 (see *Philos. Transactions*, N^o 261). I find no account of the annual burials just at that time; but from 1661 to 1681, the medium had been 1613; and from 1715 to 1728 it was 2123. There can, therefore, be no material error in supposing that in 1695 it was 1800; and this makes 1 in 22 to die annually.—In 1745 the number of *families* in the same city appeared, by an exact account laid before the Lord Mayor, to be 9,214. It is probable, this number of families did not consist of more than 50,000 individuals. Suppose them, however, 55,000; and, as at this time the medium of annual burials appears to have been 2,360; 1 in 23 died annually; see Dr. Short's *Comparative History*, p. 15, and *New Observations*, p. 228.—I know not how far these facts may be depended on. If they come at all near the truth, they demonstrate that I have been very moderate in making

supposing the annual recruit from the country to be 7000, the number of *births* 3 times 7000 or 21,000, and the *burials* and *migrations* 28,000 (which seem to be all high suppositions), the number of inhabitants will be $22\frac{1}{2}$ multiplied by 28,000, or 630,000.

I will just mention here one other instance of exaggeration on the present subject.

Mr. Corbyn Morris, in his *Observations on the past*

only 1 in $22\frac{1}{2}$, including emigrants, to die in London annually.

—In 1631 the number of people in the *city and liberties* of London was taken, by order of the Privy Council, and found to be 130,178. — This account was taken five years after a plague that had swept off near a quarter of the inhabitants; and when, therefore, the town being full of recruits in the vigour of life, the medium of annual burials must have been lower than usual, and the births higher. Could, therefore, the medium of annual burials at that time, within the walls and in the 16 parishes without the walls, be settled, exclusive of those who died in such parts of the 16 parishes without the walls, as are not in the *liberties*, the proportion dying annually obtained from hence might be depended on, as rather less than the common and just proportion. But this medium cannot be discovered with any accuracy. *Graunt* estimates that two thirds of these 16 parishes are within the *liberties*; and, if this is right, the medium of annual burials in the *city and liberties* in 1631, was 5,500, and 1 in $23\frac{1}{2}$ died annually; or, making a small allowance for deficiencies in the bills, 1 in 22. — Mr. Maitland, in his *History of London*, vol. II. p. 744, by a laborious, but too unsatisfactory, investigation, reduces this proportion to 1 in $24\frac{1}{2}$; and on the suppositions, that this is the true proportion dying annually, at all times, in London, and that the deficiencies in the burials amount to 3,038 annually, he determines that the number of inhabitants within the bills was 725,903 in the year 1737.

The number of burials not brought to account in the bills is, probably, now much greater than either Dr. Brakenridge or Mr. Maitland suppose it. I have reckoned it so high as 6000, in order to include emigrants, and also to be more sure of not falling below the truth.

growth

growth and present state of the city of London, published in 1751, supposes that no more than a 60th part of the inhabitants of London, who are above 20, die every year, and from hence he determines that the number of inhabitants was near a million. In this supposition there was an error of at least one half. According to Dr. Halley's Table, it has been shewn, that a 34th part of all at 20 and upwards, die every year at Breffaw. In London, a 29th part, according to Mr. Simpson's Table, and also according to all other Tables of London Observations. And in *Scotland* it has been found for many years, that of 974 ministers and professors whose ages are 27 and upwards, a 33d part have died every year. Had, therefore, Mr. Morris stated a 30th part of all above 20 as dying annually in London, he would have gone beyond the truth, and his conclusion would have been 400,000 less than it is.

Dr. Brakenridge observed, that the number of inhabitants, at the time he calculated, was 127,000 less than it had been. The bills have lately advanced, but still they are much below what they were from 1717 to 1743. The medium of the annual *births*, for 20 years, from 1716 to 1736, was 18,000, and of *burials* 26,529; and by calculating from hence on all the same suppositions with those which made 651,580 to be the present number of inhabitants in London, it will be found that the number then was 735,840, or 84,260 greater than the number at present. London, therefore, for the last 30 years, has been decreasing; and though now it is increasing again, yet there is reason to think that the additions lately made to the number of

buildings round it, are owing, in a great measure, to the increase of luxury, and the inhabitants requiring more room to live upon*.

It should be remembered, that the number of inhabitants in London is now so much less as I have made it, than it was 40 years ago, on the supposition that the proportion of the omissions in the *births* to those in the *burials* was the same then that it is now. But it appears that this is not the fact.—From 1728, the year when the ages of the dead was first given in the *bills*, to 1742, near five-sixths of those who were born died under 10, according to the *bills*.—From 1742 to 1752 three quarters; and ever since 1752 this proportion has stood nearly as it is now, or at somewhat more than two-thirds. The omissions in the *births*, therefore, compared with those in the *burials* were greater formerly; and this must render the difference between the number of inhabitants now and formerly less considerable than it may seem to be from the face of the bills. One reason why the proportion of the amounts of the *births* and *burials* in the bills comes now nearer than it did to

* The medium of annual burials in the 97 parishes within the walls was,

From 1655 to 1664,	_____	3264
From 1680 to 1690,	_____	3139
From 1730 to 1740,	_____	2316
From 1758 to 1768,	_____	1620

This account proves, that though, since 1655, London has doubled its inhabitants, yet, *within the walls*, they have decreased; and so rapidly for the last 30 years as to be now reduced to one half.—The like may be observed of the 17 parishes immediately without the walls. Since 1730 these parishes have been decreasing so fast, that the *annual burials* in the

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the true proportion, may, perhaps, be that the number of Dissenters is considerably lessened. The Foundling Hospital also may have contributed a little to this event, by lessening the number given in the bills as having died under 10, without taking off any from the *births*; for all that die in this hospital are buried at *Pancrass* church, which is not within the *bills*. See the preface to a collection of the yearly bills of mortality from 1657 to 1758 inclusive, p. 15.

I will add, that it is probable that London is now become less fatal to children than it was; and that this is a further circumstance which must reduce the difference I have mentioned; and which is likewise necessary to be joined to the greater deficiencies in the births, in order to account for the very small proportion of children who survived 10 years of age, during the two first of the periods I have specified. Since 1752, London has been thrown more open. The custom of keeping country-houses, and of sending children to be nursed in the country, has prevailed more. But, particularly, the destructive use of spirituous liquors among the poor has been checked.

I have shewn that in London, even in its present

them have sunk from 8,672 to 5,432, and are now lower than they were before the year 1660. In Westminster, on the contrary, and the 23 out-parishes in Middlesex and Surrey, the *annual burials* have, since 1660, advanced from about 4000 to 16,000.—These facts prove that the inhabitants of London are now much less crowded together than they were. It appears, in particular, that *within the walls* the inhabitants take as much room to live upon as double their number did formerly.—The very same conclusions may be drawn from an examination of the *christenings*.

state,

state, and according to the most moderate computation, half the number born die under *three* years of age; and I have observed that at Breslaw half live to 16. At Edinburgh, if I may judge from such of its bills as I have seen, almost as great a proportion of children die as even in London. But it appears from *Graunt's** accurate account of the births, weddings, and burials in three country parishes for 90 years; and also, with abundant evidence, from Dr. Short's collection of observations in his *Comparative History*, and his treatise entitled, *New Observations on Town and Country Bills of Mortality*†; that in country villages and parishes, the major part live to mature age, and even to marry. So great is the difference, especially to children, between living in great towns and in the country. But nothing can place this observation in a more striking light than the curious account given by Dr. Thomas Heberden, and published in the Philosophical Trans-

* See *Natural and Political Observations on the Bills of Mortality*, by Captain John Graunt, F. R. S.

† The public is much obliged to this author for the pains he has taken in collecting observations on the mortality and increase of mankind, in different countries and situations. In his *New Observations*, p. 309, he mentions an ingenious parish clerk, in the country, who, by a particular account which he took, found that of 314, who had been baptized in his parish in one year, 80, or nearly a quarter part, died under four years of age. Forty-six died the first year; thirteen the second; sixteen the third; and five the fourth. After four, life grows more stable, and at ten acquires its greatest stability; and in this case it cannot be reckoned that above a 10th, or, at most, an 8th more than the quarter that died under four, would die under age; and therefore, probably, near two-thirds arrived at maturity.

actions (vol. LVII. p. 461), of the increase and mortality of the inhabitants of the island of Madeira. In this island, it seems, the weddings have been to the births, for 8 years, from 1759 to 1766, as 10 to 46.8; and to the burials as 10 to 27.5. Double these proportions, therefore, or the proportion of 20 to 46.8, and of 20 to 27.5 are the proportions of the number marrying annually, to the number born and the number dying. Let 1 marriage in 10 be a 2d or 3d marriage on the side of either the man or the woman, and 10 marriages will imply 19 individuals who have grown up to maturity, and lived to marry once or oftener; and the proportion of the number marrying annually the first time, to the number dying annually, will be 19 to 27.5, or near 3 to 4. It may seem to follow from hence, that in this island near three-fourths of those who die have been married, and, consequently, that not many more than a *quarter* of the inhabitants die in childhood and celibacy; and this would be a just conclusion were there no increase, or had the births and burials been equal. But it must be remembered, that the general effect of an increase, while it is going on in a country, is to render the proportion of persons marrying annually to the annual deaths *greater*, and to the annual births *less* than the true proportion marrying out of any given number born. This proportion generally lies between the other two proportions, but always nearest to the first*; and, in the present case, it is sufficiently evident that it cannot be much less than two-thirds.

* In a country where there is no increase or decrease of the inhabitants, and where also life, in its first periods, is so stable,

In London, then, *half* die under three years of age, and in Madeira about *two-thirds* of all who are

and marriage so much encouraged, as that half all who are born live to be married, the *annual* births and burials must be equal, and also *quadruple* the number of weddings, after allowing for 2d and 3d marriages. Suppose in these circumstances (every thing else remaining the same) the *probabilities of life*, during its first stages, to be improved. In this case, more than *half* the born will live to be married, and an increase will take place. The births will exceed the burials, and both fall below *quadruple* the weddings; or, which is the same, below *double* the number annually married.——Suppose next (the *probabilities of life* and the *encouragement to marriage* remaining the same) the *prolifickness* only of the marriages to be improved. In this case it is plain, that an increase also will take place; but the *annual* births and burials, instead of being less, will now both rise above *quadruple* the weddings, and therefore the proportion of the born to that part of the born who marry (being by supposition two to one) will be less than the proportion of either the *annual* births or the *annual* burials to the number marrying *annually*.——Suppose again (the *encouragement to marriage* remaining the same) that the *probabilities of life* and the *prolifickness of marriages* are both improved. In this case, a more rapid increase will take place, or a greater excess of the births above the burials; but at the same time they will keep nearer to *quadruple* the weddings, than if the latter cause only had operated, and produced the same increase.——I should be too minute and tedious, were I to explain these observations at large. It follows from them, that, in every country or situation where, for a course of years, the *burials* have been either *equal to* or *less* than the *births*, and both under *quadruple* the marriages; and also that wherever the burials are *less* than quadruple the annual marriages, and at the same time the births *greater*, there the major part of all that are born live to marry. In the instance which I have considered above, and which occasions this note, the annual births are so much *greater* than *quadruple* the marriages, and at the same time the annual burials so much *less*, that the proportion that lives to marry of those who are born can scarcely be much less than I have said, or two-thirds.

born live to be married. Agreeably to this, it appears also from the account I have referred to, that the *expectation* of a child just born in Madeira is about 39 years, or near double the expectation of a child just born in London. For the number of inhabitants was found, by a survey made in the beginning of the year 1767, to be 64,614. The annual medium of *burials* had been, for eight years, 1293; of *births* 2201. The number of inhabitants, divided by the annual medium of *burials*, gives 49.89, or the *expectation* nearly of a child just born, supposing the *births* had been 1293, and constantly equal to the *burials*, the number of inhabitants remaining the same. And the same number, divided by the annual

I have shewn how the allowance is to be made for 2d and 3d marriages; but it is not so considerable as to be of any particular consequence; and, besides, it is, in part, compensated by the natural children which are included in the births, and which raise the proportion of the births to the weddings higher than it ought to be, and therefore bring it nearer to the true proportion of the number born *annually*, to those who marry annually, after deducting those who marry a 2d or 3d time.

In drawing conclusions from the proportion of *annual* births and burials in different situations, some writers on the increase of mankind have not given due attention to the difference in these proportions arising from the different circumstances of increase or decrease among a people. One instance of this I have now mentioned; and one further instance of it is necessary to be mentioned. The proportion of *annual* births to weddings has been considered as giving the true number of children derived from each marriage, taking all marriages one with another. But this is true only when, for many years, the births and burials have kept nearly equal. Where there is an excess of the births occasioning an increase, the proportion of *annual* births to weddings must be less than the proportion of children derived from each marriage; and the contrary must take place where there is a decrease.

medium of *births*, gives 29.35, or the *expectation* of a child just born, supposing the burials 2201, the number of births and of inhabitants remaining the same; and the true *expectation* of life must be somewhere near the mean between 49.89 and 29.35.

Again: A 50th part of the inhabitants of Madeira, it appears, die annually. In London, I have shewn, that above twice this proportion dies annually. In smaller towns a smaller proportion dies, and the births also come nearer to the burials. At Breslaw, I have observed, that, by Dr. Halley's Table, a 28th part dies annually; and the annual medium of births, for a complete century, from 1633 to 1734, has been 1089; of burials 1256. * At Norwich, the annual medium of births, dissenters included, for four years, from 1751 to 1754, was 1150; of burials 1214. And as the number of inhabitants was at that time 36,169 (see pag. 103), a 30th part of the inhabitants died annually. In general, there seems reason to think that in towns (allowing for particular advantages of situation, trade, police, cleanliness, and openness, which some towns may have), the excess of the burials above the births and the annual deaths are more or less as the towns are greater or smaller. In London itself, about 160 years ago, when it was scarcely a fourth part of its present bulk, the births were nearly equal to the burials.

* Vid. Dr. Short's *Comparative History*, p. 63. And the *Abridgment of the Philosophical Transactions*, vol. VII. part iv. p. 46. During the five years on which Dr. Halley has founded his Table, or from 1687 to 1691, the births happened a little to exceed the burials.

But in country parishes and villages the births almost always exceed the burials; and I believe it seldom happens that so many as a 30th, or much more than a 40th part of the inhabitants die annually*. In the four provinces of New England there is a very rapid increase of the inhabitants: but, notwithstanding this, at Boston, the capital, the inhabitants would decrease were there no supply from the country: for, if the account I have seen is just, from 1731 to 1762, the burials have all along exceeded the births†. So remarkably do towns, in consequence of their unfavourableness to health, and the luxury which generally prevails in them, check the increase of countries.

* In 1738 there was an account taken of the number of families and inhabitants in the Prussian dominions. The number of inhabitants was 2,138,465. The medium of annual births, weddings, and burials was nearly 84,000; 21,000, and 55,481. Near a 40th part, therefore, died every year. Vid. Dr. Snort's *Comparative History*, p. 69, and *Abridgment of the Philosophical Transactions*, *ibid.*—The proportion of weddings and burials to the births shews that, in these countries, there was a quick increase, notwithstanding the waste in the cities.—In the year 1733 a survey was taken of the inhabitants of the parish of *Stoke Damerel* in *Devonshire*, and the number of men, women, and children, was found to be 3361.—The *christenings* for the year were 122—the *weddings* 28—the *burials* 62.—No more, therefore, than the 54th part of the inhabitants died in the year.—In part of this year an epidemical fever prevailed in the parish. See Martyn's *Abridgment of the Philos. Transactions*, vol. IX. p. 325.—According to Graunt's account of a parish in *Hampshire*, not reckoned, he says, remarkably healthful, a 50th part of the inhabitants had died annually for 90 years. *Natural and Political Observations*, &c. Chap. xii.

† See a particular account of the births and burials in this town from 1731 to 1752 in the *Gentleman's Magazine* for 1753. p. 413.

Healthfulness and Prolifickness are, probably, causes of increase seldom separated. In conformity to this observation, it appears from comparing the births and weddings, in countries and towns where registers of them have been kept, that in the former, marriages, one with another, seldom produce less than four children each; generally between four and five, and sometimes above five. But in towns seldom above four; generally between three and four; and sometimes under three*.

I have sometimes heard the great number of old people in London mentioned to prove its favourableness to health and long life. But no observation can be much more erroneous. There ought, in reality, to be more old people in London, in proportion to the number of inhabitants, than in any smaller towns, because at least one quarter of its inhabitants are persons who come into it, from the country, in the most robust part of life, and with a much greater probability of attaining old age, than if they had come into it in the weakness of infancy. But, notwithstanding this advantage, there are much fewer persons who attain to great ages in London than in any other place where observations have been made.—— At Vienna, of 22,704 who died in the four years

* Any one may see what evidence there is for this, by consulting the accounts in Dr. Short's two books already quoted; and in the *Abridgment of the Philosophical Transactions*, vol. VII. part iv. p. 46.—In considering these accounts, it should not be forgotten that allowances must be made for the different circumstances of increase or decrease in a place, agreeably to the observation at the end of the note in pag. 113.

1717, 1718, 1724, 1725 *, 109 reached 90 years, that is, 48 in 10,000. But in London, for the last 30 years, only 35 of the same number have reached this age.—At Breslaw it appears, by Dr. Halley's Table, that 41 of 1238 born, or a 30th part, live to be 80 years of age.—In the parish of *All-saints* in Northampton†, an account has been kept for many years of the ages at which all die; and, I find, that of 1377, who died there in 13 years, 59 have lived to be 80, or a 23d part.—According to Mr. Kerffboom's Table of Observations, published at the end of the last edition of Mr. De Moivre's Treatise on the Doctrine of Chances, a 14th part of all that are born live to be 80; and, had we any observations in *country* parishes, this, probably, would not appear to be too high a proportion‡. But in London, for the last 30 years, only 25 of every 1000

* Vid. Abridgment of the Philosophical Transactions, vol. VII. part iv. p. 46. — It appears also that more than three-fifths of all who died in these years at Vienna were boys and girls, by whom, I suppose, are meant persons under 16. About the same proportion dies under 16 at Berlin.

† In this town, as in most other towns of any magnitude, the births, including Dissenters, fall short of the burials; and the greater part die under age.

‡ This, however, will appear itself inconsiderable, when compared with the following account: "In 1761, the burials in the district of Christiana, in Norway, amounted to 6,929, and the christenings to 11,024. Among those who died, 394, or 1 in 18, had lived to the age of 90; 63 to the age of 100, and seven to the age of 101.—In the diocese of Bergen, the persons who died amounted only to 2,580, of whom 18 lived to the age of 100; one woman to the age of 104, and another woman to the age of 108."

See the *Annual Register* for 1761, p. 191.

who

who have died, have lived to be 80, or a 40th part ; which may be easily discovered by dividing the sum of all who have died during these years at all ages, by the sum of all who have died above 80.

Among the peculiar evils to which great towns are subject, I might further mention the PLAGUE. Before the year 1666 this dreadful calamity laid London almost waste once in every 15 or 20 years ; and there is no reason to think that it was not generally bred within itself. A most happy alteration has taken place, which, perhaps, in part, is owing to the greater advantages of cleanliness and openness, which London has enjoyed since it was rebuilt, and which lately have been very wisely improved.

The facts I have now taken notice of are so important that, I think, they deserve more attention than has been hitherto bestowed upon them. Every one knows that the strength of a state consists in the number of people. The encouragement of population, therefore, ought to be one of the first objects of policy in every state ; and some of the worst enemies of population are the luxury, the licentiousness, and debility produced and propagated by great towns.

I have observed that London is now* increasing. But it appears that, in truth, this is an event more to be dreaded than desired. The more London in-

* This increase is greater than the bills shew, on account of the omission in them of the two parishes which have been most increased by new buildings ; I mean *Marybone* and *Pancras* parishes. The former of these parishes is, I suppose, now one of the largest in London.

creases, the more the rest of the kingdom must be deserted; the fewer hands must be left for agriculture; and, consequently, the less must be the plenty and the higher the price of all the means of subsistence. — *Moderate* towns, being seats of refinement, emulation, and arts, may be public advantages. But *great* towns, long before they grow to half the bulk of London, become checks on population of too hurtful a nature, nurseries of debauchery and voluptuousness; and, in many respects, greater evils than can be compensated by any advantages*.

* The mean annual *births*, *weddings*, and *burials* in the following towns, for some years before 1768, were nearly,

		Births.		Weddings.		Burials.
At Paris,	—	19,200	—	4,300	—	19,500
Vienna,	—	5,600	—	—	—	6,800
Amsterdam,	—	4,500	—	2,400	—	7,600
Copenhagen,	—	2,700	—	868	—	3,100

In the Paris bills there is, I am informed, an omission of all that die in the *Foundling Hospital*, amounting to above 2000 annually. The excess, therefore, of the burials above the births is greater than the bills shew. This excess, however, is much less than could have been expected in so large a town. I am not sure to what cause this ought to be ascribed; but I cannot wonder at it, if it be indeed true, that a fifth of all born in Paris are sent to the *Foundling Hospital*, and that a third of the inhabitants die in *hospitals*, and also that all married men are excused from serving in the militia, from whence draughts are made for the army. These are encouragements to marriage and population, which no other city enjoys; and it is strange that in this kingdom some policy of the same kind with that last mentioned should not be pursued. — A further singularity in the state of Paris is, that the births in it are above four times the weddings, nothing like which is the case in any other town whose bills I have seen. It may seem, therefore, that here, as well as in the most healthful

Dr. Heberden

Dr. Heberden observes that, in Madeira, the inhabitants double their own number in 84 years. But

and increasing country parishes, each marriage produces more than four children; but this is a conclusion which, in the present case, cannot be depended on. It should be considered that, probably, some who leave the country to settle at Paris, come to it already married; and that no small proportion of the births may be illegitimate. These causes, however, may only balance the allowance to be made for the second and third marriages among the annual weddings; and, if it is indeed fact, that the people at Paris are so prolific as they appear to be in the bills, it will only prove more strongly that, like other great towns, it is very unfavourable to health; for the more prolific a people are, the greater must be the mortality among them if they do not increase.

—Let us suppose the true number of deaths at Paris, including emigrants and such as die in the *Foundling Hospital*, to be 21,000; the number married annually $2 \times 4,300$ or 8,600; and the births, as before, 19,200. 1,900 then will be the number of annual recruits from the country. Of these let only 1,200 be supposed to marry: and 8,600 lessened by 1,200, or 7,400, will be the number of those born at Paris who marry annually; and 11,800, or above *three-fifths* will be the number dying in childhood and celibacy. This, though it gives an unfavourable representation of Paris when compared with the country, makes it appear to advantage when compared with some other great towns. I am not sufficiently informed of the state of Paris to know how near this calculation comes to the truth. Every such doubt would be removed, were the ages of the dead given in the Paris bills. It is much to be wished this was done. The births and burials here come so near to one another, that there can scarcely be a proper place for such bills; and a Table of Observations might be formed from them that would give the values of lives much more exactly than the London Tables.

I cannot help adding that, excepting the omission I have mentioned in the burials, the Paris bills are complete; but it is well known that the London bills are extremely otherwise. London, therefore, must be much larger in comparison of Paris than it appears to be in the bills.

this

this (as you, Sir, well know) is a very flow increase compared with that which takes place among our colonies in AMERICA. In the back settlements, where the inhabitants apply themselves entirely to agriculture, and luxury is not known, they double their own number in 15 years; and all through the northern colonies in 25 years*. This is an instance of increase so rapid as to have scarcely any parallel. The births in these countries must exceed the burials much more than in Madeira, and a greater proportion of the born must reach maturity.—In 1738, the number of inhabitants in New Jersey was taken by order of the government, and found to be 47,369. Seven years afterwards the number of inhabitants was again taken, and found to be increased, by procreation only, above 14,000, and very near one *half* of the inhabitants were found to be under + 16 years of age. In 22 years, therefore, they must have doubled their own number, and the births must have exceeded the burials 2000 annually. As the increase here is much quicker than in Madeira, we may be sure that a smaller proportion of the inhabitants must die annually. Let us, however, suppose it the same, or a 50th part. This will make the annual burials

* See a discourse on *Christian union*, by Dr. Styles, Boston, 1761, p. 103. 109, &c.—See also *The interest of Great Britain considered with regard to her Colonies, together with Observations concerning the increase of mankind, peopling of countries, &c.* p. 35. 2d edit. London, 1761.

* According to Dr. Halley's Table the number of the living under 16 is but a *third* of all the living at all ages; and this may be nearly the case in all places which just support themselves in the number of their inhabitants, and neither increase or decrease.

to have been, during these seven years, 1000, and the annual births 3000, or an 18th part of the inhabitants.—Similar observations may be made on the much quicker increase in Rhode Island, as related in the preface to Dr. Birch's *collection of the bills of mortality*, and also in the valuable pamphlet, last quoted, on *the interest of Great Britain with regard to her colonies*, p. 36.—What a prodigious difference must there be between the vigour and the happiness of human life in such situations, and in such a place as London?—The original number of persons who, in 1643, had settled in New England, was 21,200. Ever since it is reckoned, that more have left them than have gone to them*. In the year 1760 they were increased to half a million. They have, therefore, all along doubled their own number in 25 years; and, if they continue to increase at the same rate, they will, 70 years hence, in New England alone, be four millions; and in all North America above twice the number of inhabitants in Great-Britain†.—But I am wandering

* See Dr. Styles's pamphlet just quoted, p. 110, &c.

† The rate of increase, supposing the procreative powers the same, depends on two causes: The "encouragement to marriage;" and the "*expectation* of a child just born." When one of these is given, the increase will be always in proportion to the other. That is; As much *greater* or *less* as the *ratio* is of the numbers who reach maturity, and of those who marry to the number born, so much *quicker* or *slower* will be the increase.—Let us suppose the operation of these causes such as to produce an annual excess of the *births* above the *burials* equal to a 36th part of the whole number of inhabitants. It may seem to follow from hence, that the inhabitants would double their own number in 36 years; and thus some have calculated. But the truth is, that they would double their own number in much less time.

from

from my purpose in this letter. The point I had chiefly in view was, the present state of London as

Every addition to the number of inhabitants from the births produces a proportionably greater number of births, and a greater excess of these above the burials; and if we suppose the excess to increase annually at the same rate with the inhabitants, or so as to preserve the *ratio* of it to the number of inhabitants always the same, and call this *ratio* $\frac{1}{r}$, the period of doubling will be the *quotient* produced by dividing the logarithm of 2 by the *difference* between the logarithms of $r+1$ and r , as might be easily demonstrated. In the present case, r being 36, and $r+1$ being 37, the period of doubling comes out 25 years. If r is taken equal to 22, the period of doubling will be 15 years.—But it is certain that this ratio may, in many situations, be greater than $\frac{1}{22}$; and, instead of remaining the same, or becoming less, it may *increase*, the consequence of which will be, that the period of doubling will be shorter than this rule gives it.—According to Dr. Halley's Table, the number of persons between 20 and 42 years of age is a third part of the whole number living at all ages. The prolific part, therefore, of a country may very well be a 4th of the whole number of inhabitants; and supposing four of these, or every other marriage between persons all under 42, to produce *one* birth every year, the annual number of births will be a 16th part of the whole number of people; and, therefore, supposing the burials to be a 48th part, the annual excess of the births above the burials will be a 24th part, and the period of doubling 17 years.—The number of inhabitants in New England was, as I have said from Dr. Stiles's pamphlet, half a million in 1760. If they have gone on increasing at the same rate ever since, they must be now 640,000; and it seems to appear that in fact they are now more than this number. For, since I have writ the above observations, I have seen a particular account, grounded chiefly on surveys lately taken with a view to taxation and for other purposes, of the number of males, between 16 and 60, in the four provinces. According to this account, the number of such males is 218,000. The whole number of people, therefore, between 16 and 60, supposing 14 males to 13 females, must be nearly

to healthfulness, number of inhabitants, and its influence on population. The observations I have made may, perhaps, help to shew how the most is to be made of the lights afforded by the London bills, and serve as a specimen of the proper method of calculating from them. It is indeed extremely to be wished that they were less imperfect than they are, and extended further. More parishes round London might be taken into them; and, by an easy improvement in the parish registers now kept, they might be

420,000. In order to be more sure of avoiding excess, I will call them only 400,000. In Dr. Halley's Table the proportion of all the living under 16 and above 60, to the rest of the living, is 13.33 to 20; and this will make the number of people now living in the four provinces of New England to be 666,000. But, on account of the rapid increase, this proportion must be considerably greater in New England, than that given by Dr. Halley's Table. In New Jersey, I have said the number of people under 16 was found to be almost equal to the number above 16. Suppose, however, that in New England, where the increase is somewhat slower, the proportion I have mentioned is only 16 to 20, and then the whole number of people will be 720,000.

I cannot conclude this note without adding a remark to remove an objection which may occur to some in reading Dr. Herberden's account of Madeira, to which I have referred. In that account 5945 is given as the number of children under seven in the island, at the beginning of the year 1767. The medium of annual births, for eight years, had been 2201; of burials 1293. In six years, therefore, 13,206 must have been born; and if, at the end of six years, no more than 5945 of these were alive, 1210 must have died every year. That is; almost all the burials in the island, for six years, must have been burials of children under seven years of age. This is plainly incredible; and, therefore, it seems certain, that the number of children under seven years of age must, through some mistake, be given, in that account, 3000 or 4000 too little.

extended

extended through all the parishes and towns in the kingdom. The advantages arising from hence would be very considerable. It would give the precise law according to which human life wastes in its different stages, and thus supply the necessary *data* for computing accurately the values of all *life-annuities* and *reversions*. It would, likewise, shew the different degrees of healthfulness of different situations, mark the progress of population from year to year, keep always in view the number of people in the kingdom, and, in many other respects, furnish instruction of the greatest importance to the state. Mr. De Moivre, at the end of his book on the doctrine of chances, has recommended a general regulation of this kind; and observed, particularly, that at least it is to be wished, that an account was taken, at proper intervals, of all the living in the kingdom, with their ages and occupations; which would, in some degree, answer most of the purposes I have mentioned.—But, dear Sir, I am sensible it is high time to finish these remarks. I have been carried in them far beyond the limits I at first intended. I always think with pleasure and gratitude of your friendship. The world owes to you many important discoveries; and your name must live as long as there is any knowledge of philosophy among mankind. That your happiness in this, and every other respect, may continually increase, is the sincere wish of,

SIR,

Your much obliged,
and very humble servant,

Newington-Green,
April 3, 1769.

Richard Price.

XVII. *Dissertation*

XVII. *Dissertatio epistolaris de Ossibus et Dentibus Elephantum, aliarumque Belluarum in America Septentrionali, aliisque borealibus Regionibus obviis; qua indigenarum Belluarum esse ostenditur. Auctore R. E. Raspe, Serenissimo Hassiarum Landgravio à Consiliis, et R. S. S.*

SOCIETATI REGIÆ SCIENTIARUM
LONDINENSIS.

Read May 11, 1769. **P**ERSCRIPSIT ad me illustris Pringlius, baronettus: “ Media nuper in America Septentrionali, non præcul ab Ohio fluvio, intra spatium duorum aut trium jugerum terræ paludosæ, fale foecundæ, sceleta triginta belluarum majorum inventa fuisse, nulla aut rara terra solum sepulta: Si magnitudinis offium atque dentium majorum habeatur ratio, elephantina videri; ob formam molarium dentium, qui carnivori potius sint animalis, ambigi cui belluarum speciei tribuenda; quæri inter naturæ studiosos de phænomeni causis; pro sua in me benevolentia et meam rogare sententiam.”

Hanc dissertationem, quamvis difficultates quæstionis magis ostendat quam solvat, malui judicio vestro potius

potius subijcere quam dare aliud quodvis studiorum meorum documentum.

Duplex proprie est quæstio; altera, “ de causa, quæ harum belluarum ossa in Americam Septentrionalem transvexerit?” Altera, “ quæ in tam brevi spatio concluderit et sepelierit?”

Primam soluturo occurrit:

1. Historicorum Americæ constans observatio, omnem hanc novi orbis terram, cum tribus fere abhinc seculis detecta Europæis patere cœpit, nonnisi à quadrupedibus minoribus fuisse habitatam; peregrina igitur videri illa, sive elephantum sive aliarum belluarum colossalia sint ossa.

2. In mentem redeunt ossa majora in Asiæ aut Sibiariæ borealibus regionibus inventa et obvia, quæ subterraneo lucifugo animali, quod Mammout dicitur, tribuit fabula, elephantis aliisque belluis vindicant commentarii Petropolitani et Gmelinus in Itinerario Sibiariaco. Nec in Asiæ et Americæ solum borealibus provinciis ea occurrunt, effodiuntur etiam in Europæ climatibus septentrionalibus.

De Britannicis huc pertinentia quædam in Transactionibus vestris legisse memini. De Suecia fidem faciunt Holmiensis academici commentarii. Ex Germaniæ naturali historia minus vulgatas aliquot observationes addere liceat.

Nil moror gigantum ossa, quæ credula antiquitas spectanda servavit in cathedralibus quibusdam templis, ut Halberstadenfi, Ganderstæmenfi, aliisque. Huc facere quidem videntur, sed certiora repeto et primo quidem ex electoratus et ducatus Brunsvicensis naturali historia, diligentius olim à me perscrutata. Recordemini ossium in antris Baumanniano, Schartzfeldensi, et prope Dü-

nam

nam in Hercynia effossorum. Ea scriptis eruditorum Leibnitzii in *Protegea* et Hollmanni in Commentariis Goettingensibus celebrata sunt, atque cuilibet insipienti belluarum majorum terrestrium esse videntur, ex parte rhinocerotum ossibus persimilia ab Hollmanno demonstrata sunt. Dentem majorem, elephantinis forma et substantiæ textura exacte convenientem et ad Fischbeckam prope Hamelam ad Visurgim inventum, quem Hannoveræ in instructissimo suo museo servat mihi amicissimus Andreæ, plus una vice manibus versavi. Vidi etiam Blumenauiæ prope Hannoveram effossam majorem costam incerti animalis. De specie belluæ majoris ad Quedlinburgum effossæ, cujus meminit Guerickius Magdeburgensis consul in libro *de vacuo*, quam maxime anceps est quæstio. Certius pronunciare licet de dentibus, in fundamentis jaciendis urbis Neo-Cassellensis, quam nunc inhabito et per Serenissimi Landgravii Hassiarum in me cumulatam et liberalitatem et gratiam alteram prædico patriam, effossis. Isti elephantini sunt; et cum aliis naturæ et artis miraculis in museo publico, mihi ex parte concredito, servantur. Plura non addo exempla, non quod deficiat materies, nam et antiquitas Græca et Romana et quicquid fere est cultiorum populorum similia adnotavit; adducta vero sufficiunt ad probandum “ in omnibus borealibus regionibus et antiqui et novi orbis inveniri majorum belluarum ossa, quæ vivæ in illis non supersunt, et per cœli inclementiam nunquam in illis generasse videntur.”

3. Subit accuratius perpendendus situs et natura soli in quibus deprehendi solent.

In stratis lapideis, corporibus marinis mixta, inclusa, tecta inveniri nusquam legi.

Delitescant in cavernis montium ut in tribus illis Hercyniæ antris; aut palude, aut terra hortensi, aut limo exterioris superficiei terræ vix contacta atque involuta jacent, quod de Americanis, Sibiriacis, aliisque, ex relationibus fide dignis, constat.

Nec etiam, ut corpora marina saxis inclusa, lapidis duritiem et naturam induerunt. Diversimode calcinata, parum aut nihil mutata sunt.

In Europæ septentrionalibus plagis rarius jam et solitaria sere deteguntur, forte quod exhausta curiositatis materies, et quod diutius habitata a populis miraculorum et medicamentorum appetentibus, nam ossa hujusmodi, eboris aut unicornu fossilis nomine, in officinis pharmacopœorum prostant, et nescio quam illis antidoti mirabilem vim tribuerat antiquitas.

In ipsis quoque Americæ et Sibiariæ terris, habitatione marinorum corporum in lapidem versorum immensæ copię, rariora sunt; et minor utique belluarum terrestrium progenies in natali earum terra, quasi secus non suffectura, observata fuit.

Quæ omnia si recte animo colligantur duplicem eamque ad rem facientem suspicionem movent; alteram " alio plane modo hæc ossa sepulta esse, quam
" quæ profundius in lapidibus latent corpora mari-
" na; alteram non esse peregrinas et marinas belluas
" hæc, quarum sunt partes, sed fato suo, aut ho-
" minum vi, illas in ipsis hisce borealibus terris,
" olim illis natalibus, extinctas fuisse."

Quæstio igitur proposita, jamque solvenda, est de generali quadam causâ, quæ has belluas, aut sub sole

tantum calente degentes, in Septentrionem transtulerit, aut exstinxerit similes illis species olim Septentrionis incolas.

Si peregrinas alioque advectas et cœli solum calidioris progeniem statuamus, omnis definitio periculosa videtur.

Terræ motuum et Vulcanorum vis, quæ insulas produxit et montes, et rationem sufficientem reddit stratorum lapideorum, variè inclinatorum et ruptorum, illisque inclusorum marinorum corporum, nil valet ad advehendas orientis et meridiei bestias. Superficiem terræ fundique marium mutat. Climata inconcussa manent.

Diluvii universalis Noachici aut Deucalionei cuiusdam effectibus, præter alia multa, quod supra notavi, obstat, ossa hæc vel in cavernis montium, solitis ferarum latebris, aut paludibus aut superiore solum terra, solitaria quadammodo, nunquam corporibus marinis tecta, mixta, inclusa deprehendi. Nec etiam, quæcunque tam mirabili aquarum super omnem continentem effusioni tribuatur vis et potentia, concipi animo potest, quare ossa et cadavera hæc graviora ex meridionalibus regionibus in septentrionales transexerit, eaque in alta potius mediaque continente terra deposuerit, quam in oceani abyssi, nisi forte cum Woodwardo perplaceat diluvii tempore nullam omnino gravitatis fuisse actionem, quod quidem per me licet, neminem vero vestrum rapiet in assensum.

Si recurramus ad antiquitatis remotissimæ heroas, belluis uti belli instrumentis utentes, easque invitas cum militibus suis æque forte invitis in ultimam Europam, Sibiriam, atque Americam vi, metuque propellentes, quæstionem non solvimus sed impedimus.

America enim toto oceano ab antiquo orbe divisa, et quicquid dicant veteres, navalis eorum scientia exercitiis cum elephantis in Americam ex Asia aut Europa transvehendis non sufficit. Quatenus etiam lux est in antiquorum temporum historia et moribus, sero quadrupedes in bellum domarunt, elefantos primò nonnisi Asiae et Africae reges. Tenebris vero historiarum, quæ maximam antiqui orbis partem premunt, ad fabulas novas fingendas meo quidem iudicio abuti non licet, nisi forte de quibuscunque historiae et orbis terris incognitis impune mentiri liceat.

Peregrinas igitur non esse nec aliò advectas has Septentrionis belluarum majorum reliquias quammaxime prona est conjectura; idque et illi sensere, qui climata terræ mutata, eoque ipso omnem controversiam definiri posse putarunt. Vereor autem ne eorum hypotheses, nimio autorum ingenio, collapsuræ sint.

Climata terræ, eodem orbitæ, qua circum solem revolvitur, diametro servato, duplici modo mutari atque affici posse videntur; aut magis imminuta istius orbitæ sive ecliptices obliquitate magisque ad planum æquatoris inclinata; aut axes terræ centrique gravitatis sumta quadam mutatione, quæ cum climatibus simul et terræ figuram mutat.

Prior hypothesis mirè arrisit recentioribus quibusdam philosophis, tum quod problema propositum apparenter solvat, tum etiam quod talis quædam orbitæ vel ecliptices inclinatio ejusque obliquitatis diminutio revera accidit, si quidem Eratosthenis atque Hipparchi, novemdecim ab hinc seculis institutis, observationibus sua fides constat. Illi eclipticam ab æquatoris plano suo tempore $23^{\circ} 51' 20''$ recedere notarunt,

runt, quam celeberrimus De la Caille nostris diebus $22^{\circ} 59' \frac{1}{2}$ minorem observavit in Promontorio Bonæ Spei, uti quidem notavit in *Leç. Elem. Astron.* Nolo contra antiquarum observationum fidem disputare, nec repetere quæ de incertitudine annuarum legum hujus imminutæ ecliptices obliquitatis et de causâ ejus non perpetua; inter astronomos disputata sunt. Concedam potius multo majorem olim fuisse ecliptices obliquitatem; ipsâ vero mea in hujus hypotheseos patronos liberalitate vincam. Quamcunque enim majorem orbitæ obliquitatem fingere velint, nunquam tamen eò evitabunt necessariò inde provenientes tempestatum anni vicissitudines, naturæ elephantorum quammaxime contrarias. Extendant tropicos ad quinquagesimum et ultra latitudinis gradum; intendant quantum velint calorem æstivum, et certe magis intensus fiet majore orbitæ obliquitate, et magis directè solis radiis in zonas, quas inhabitamus, incidentibus, quid elephantis, quibus illa hypothesi in nostris regionibus commodas, et vivendi et generandi sedes parare satagunt, inde commodi? Non equidem video: sentio potius, idque sphæroïdea terræ figura ipsaque major ecliptices obliquitas cuilibet perpendiculari facile persuadebit, majorem hunc æstivum calorem, constante semestri vicissitudine, majus etiam frigus hibernum insecuturum, quod tantum abest ut elephantorum naturali progeniei in zonis borealibus conveniat ut potius illi quammaxime obstat.

Quod ad axeos terræ centrique gravitatis mutationem attinet, ea aut momentanea esse debuisset, aut lenta, et per gradus successiva.

Si prius; hypothesis cælum Acherontaque movet, summa miscet imis, omnemque hominum et animalium

lium viviparorum progeniem fato subitæ mortis, et sine miraculo inevitabilis involvit, in perpetuum interituram et sine novo creationis miraculo non reviviscendam; ejusmodi enim mutatio, quæ meridionales zonas olim inter tropicos fitas uno temporis momento boreæ objiciat, sine universali terræ ruina exitiali cogitari nequit, si, quod verissimum demonstravit Newtonus, rotatio orbis polos deprimit æquatorem pandit, diameterque terræ per polos ductus minor sit altero æquatoris septem et quod excurrit milliaribus Germanicis. “ Quis talia fando temperet à lacrymis !” Tam violenta etiam climatum mutatio boreæ olim incolarum cadavera sub æquatorem in meridionales terras transvehere debuisset, quod an experientia confirmetur viderint alii quibus hæc arrident.

Sed lento gradu procedere potuit illa figuræ terræ, climatum et axeos mutatio? Concedam. Sed quare non amplius procedit? Quis impedit ne, certo quodam et procedenti axeos mutationi respondente progressu, meridionales plagas cum terræ trepitatione et mugitu versus boream procedentes paullatim deprimat et mergat, boreales vero ad meridiem jam tendentes extollat oceano? Constantes sibi fingamus naturæ leges aut nullas. Non enim senescit.

Redeo igitur ad illam, quam supra jam indigitavi, suspensionem, suo fato, aut hominum vi, obiisse belluas, quarum ossa nos exercent, ubi deprehendimus, nec peregrinas esse, nec marinas. Ea sola minoribus difficultatibus laborat. Quamvis enim nec climata, nec axem terræ, nec ejus figuram mutandam propterea censeam, sunt tamen quædam quæ obstare illi videntur, quo minus pro generali aliqua phænomeni
propositi

propositi causa haberi possit; sed obstare tantummodo videntur.

Maximam difficultatem hoc parit, pro elephantinis aut meridionalium belluarum ossibus haberi; Septentrionem et omnem Americam elephantos similisque voluminis animalia non generare; et si animalium et climatum natura constans sit, et perpetua, nunquam generasse.

Totius igitur quæstionis cardo versatur circa naturam horum ossium: an reverà elephantina aut talia sint, quæ animali in septentrione degenti convenire non possint? Et mihi quidem plus quam indecisa videtur illa ossium natura; certè elephanto vindicari non possunt molares dentes, simul cum reliquis in America eodem loco inventi; nec etiam alia ossa in Sibiria et Hercyniæ cavernis obvia illi conveniunt, quod accuratius perlegenti commentarios Petropolitinos et Gmelini itinerarium, et sine præconcepta opinione inspectanti ossa, sat clarum erit. Si per anatomicam omnium quadrupedum imprimis belluarum inter se comparatam, habito respectu ad diversas illarum ætates, quæ ossium formam mutant, constaret, nullum cujuscunque animalis os alteri convenire et convenire posse, quod non constat; tum quidem coactus confiterer re verà elephantina esse quæ videntur, reliqua quæ elephantina non videntur adscribenda speciei nondum nec in India nec alibi terrarum observatæ. Quod an difficultates omnes expediat iudicate ipsi; ne enim repetam cum nulla hypothesi elephantorum meridionalium sub septentrione ossa conciliari posse, anne admittenda erit alia belluarum species nondum observata, cui molares dentes elephantis
non

non convenientes adscribantur? Species forte nullibi superstes, si quidem nobis datum est omnes nosse, omnes ritè descripsisse? Exstirpata fortè aut emortua? Aut etiam nescio in quo mundi angulo, philosopho nulli viso, adhuc delitescens?

Quæ cum ita sint, licere mihi puto omnia hæc septentrionis colossalia ossa uni solum speciei incognitæ, olim septentrionis incolæ, aut adscribere etiam diversis. Tum me reliquarum hypothesium omnium defectus non vexabunt, et magnis me difficultatibus me explicuisse putabo——“ forte ut incidam in Scyl-
“ lam volens evitare Charybdim,” quod vestrum judicium esto.

Integras ferarum species in Britannia, Germania, aliisque Europæ provinciis penitus exstirpatas, aut quam proxime emorituras ipsi nostis, aut quod no-civæ habitæ fuerint, aut in pellibus et carnibus earum luxuriatum sit genus humanum. De lupis, ursis, alce, uro, plerisque, Cæsaris ævo, in Germania Rhennana obviis sermo mihi est. Quidni igitur noxiam aliquam belluæ speciem, rarius generantem per longam seculorum seriem excindere potuisset septentrionalium populorum ab omni historiæ memoria celebratum venationis et bellicæ fortitudinis studium? Certe ea via ad gloriam ibant omnes, et Herculem et quemvis primi seculi herosa acrem monstrorum debellatorem cantat fabula. Castorum et zibellinorum speciebus simile quid nostris ferè diebus accidit, eoque minus de almæ naturæ lege, species omnes servante, contra me disputari potest; et in seculo quidem pluribus animantium speciebus fatali, et ab homine in omnium ferè specierum individua sævo. Species etiam in septentrione non superstites superesse possunt in zonis
antarcticis.

antarcticis nostris borealibus parallelis. Quis negat? quis novit?

Elephantorum Africæ, Indiæ, et Ceylanensium diversitatem quandam notant naturæ historici, Ceylanenses reliquis omnibus magnitudine et ingenio præcellere prædicantes. Esse igitur potuit elephantorum species in ipso septentrione, reliquis omnibus præcellens frigoris patientia, et à meridionalibus elephantis non magis diversa quam Lappones aut Hurones diversi esse solent a Nigritis et Malabariæ incolis. Sed certi quid pronunciare non ausim. Sufficit, omnium reliquarum hypothesium insufficientiam, naturam situs et ossium ipsorum, tandem etiam exempla extirpatarum specierum persuadere, quammaximè probabili quadam ratione, belluas quarum ossa in septentrione hinc inde latent, non esse peregrinas, non aliò advectas sed indigenas in illis terris, ipsis olim natalibus, vixisse, generasse, quamvis hodie non supersint.

Altera jam quæstio de causa, quæ tam ingentem sceletorum atque ossium acervum in tam brevi Americæ Septentrionalis spatio concluderit et sepelierit? quammaxime est expedita. Latebant in terra paludosa, sale fecunda, rara solum terra tecta; non quod hæc belluæ de salinarum, dominio et proprietate humana quadam arte et crudelitate, invicem debellarint et fortitudine sua ceciderint in fundo ad præliandum quammaxime incommodo, quod videri poterat primo obtutu et jocabundo cuidam amicorum meorum videbatur; sed quod salis liguriendi deliciis in paludem allecæ, paludosa terra ponderi corporis cedente, haustæ, impeditæ, sepultæ fuerunt, luxuriam et voluptatem morte luentes.

Hæc

Hæc fere sunt quæ ad solvendas aut impediendas supra nominatas quæstiones mihi quidem dici posse videntur, elegantius forte et doctius dicenda, si magis verborum limatorum illecebris munerumque pretio atque pondere quam dantis et scribentis animo deliniendi et capiendi foretis. Illum vero vobis addictissimum testor, sincera vota pro vestræ Societatis incolumitate et flore fundentem, vestræque salutis et veritatis omnis cupidissimum.

R. E. Raspe.

XVIII. *Observations on a particular Manner of Increase in the Animalcula of vegetable Infusions, with the Discovery of an indissoluble Salt arising from Hemp-seed put into Water till it becomes putrid.*
By John Ellis, Esq; F. R. S.

Read May 28, 1769. **H**AVING, at the request of Dr. Linnæus, made several experiments on the infusion of mushrooms in water, in order to prove the theory of Baron Munchhausen, that their seeds are first animals and then plants; which he takes notice of in his System of Nature, p. 1326, under the genus of *Cbaos*, by the name of *Cbaos fungorum seminum*: it appeared evidently that the seeds were put into motion by very minute animalcula which proceeded from the putrefaction of the mushroom; for by pecking at these seeds, which are reddish, light, round bodies, they moved them about with great agility in a variety of directions, while the little animals themselves were scarcely visible, till the food they had eaten had discovered them. The satisfaction I received from clearing up this point led me into many other curious and interesting experiments. I looked carefully over Mr. Turbervill Needham's, F. R. S. very ingenious memoir on this subject, vol. XLV. p 615, of the Philosophical Transactions:

Transactions: I mean as to the experiments, many of which succeeded with me, some not*. I own, his

* The ingenious Mr. Needham supposes, those little transparent ramified filaments, and jointed or coralloid bodies, which the microscope discovers to us on the surface of most animal and vegetable infusions when they become putrid, to be zoophytes or branched animals: but to me they appear (after a careful scrutiny, with the best glasses) to be of that class of *Fungi* called *Mucor* or Mouldiness, many of which Michelius has figured, and Linnæus has accurately described.

Their vegetation is so amazingly quick, that they may be perceived in the microscope, even to grow and feed under the eye of the observer.

Mr. Needham has pointed out to us one that is very remarkable for its parts of fructification. See Phil. Trans. vol. XLV. Tab. v. fig. 3, a, A; this, he says, proceeded from an infusion of bruised wheat.

I have seen the same species arise from the body of a dead fly, which was become putrid by lying floating for some time in a glass of water, where some flowers had been, in the month of August, 1768: This species of *Mucor* sends forth a mass of transparent filamentous roots, from whence arise hollow stems, that support little oblong-oval seed vessels with a hole on the top of each; from these I could plainly see minute globular seeds issue forth, in great abundance, with an elastic force, and turn about in the water as if they were animated.

Continuing to view them with some attention, I could just discover, that the putrid water, which surrounded them, was full of the minutest animalcula, and that these little creatures began to attack the seeds of the *Mucor* for food, as I have observed before in the experiment on the seeds of the larger kind of *Fungi* or mushrooms. This new motion continued the appearance of their being alive for some time longer: but soon after many of them arose to the surface of the water, remaining there without motion; and a succession of them afterwards coming up, they united together in little thin masses, and floated to the edge of the water, remaining there, quite inactive during the time of observation.

As this discovery had cleared up many doubts, which I had conceived from reading Mr. Needham's learned dissertation, I

reasoning is very specious and plausible, but too metaphysical for a natural historian. Yet I cannot forbear relating one of the experiments which I tried in consequence of his discovery, that animalcula were produced in various infusions, notwithstanding the greatest heat was given to the liquor.

On the 25th of May, 1768, Fahrenheit's thermometer 70 degrees, I boiled a potatoe in New-river

put into the same glass several other dead flies, by which means this species of *Mucor* was propagated so plentifully, as to give me an opportunity of frequently trying the same experiment to my full satisfaction.

Lastly, those jointed coralloid bodies, which Mr. Needham calls chaplets and pearl necklaces, I have seen frequently very distinctly. These appear not only on an infusion of bruised wheat, when it becomes putrid, but on most other bodies, that throw up a viscid scum, and are in a state of putrefaction. These then are evidently no more than the most common *Mucor*, the seeds of which are every where floating in the air; and bodies in this state afford them a proper and natural soil to grow upon. Here they send downwards their fine transparent ramified roots into the moisture which they float upon, and from the upper part of the scum their jointed coralloid branches rise full of seed into little grove-like figures. When a small portion of these branches and seeds are put into a drop of the same putrid water the scum floats upon, many of the millions of little animalcula, with which it abounds, immediately seize them as food, and turn them about with a variety of motions; as in the experiment on the seeds of the common mushrooms; either singly or two or three seeds connected together, answering exactly to Mr. Needham's description; but evidently without any motion of their own, and consequently not animated.

I am satisfied Mr. Needham's observations have convinced him long before this, that they must be vegetables: for my part, I own I have never seen a zoophyte extend its branches, and grow out of water. I hope I have already cleared up that point, in shewing the absurdity of Dr. Pallas's *Corallina terrestris*, Phil. Trans. vol. LVII. p. 415.

water till it was reduced to a mealy consistence. I put part of it, with an equal proportion of the boiling liquor, into a cylindrical glass vessel that held something less than half a wine pint, and covered it close immediately with a glass cover. At the same time, I sliced an unboiled potatoe, and, as near as I could judge, put the same quantity into a glass vessel of the same kind, with the same proportion of New-river water, not boiled, and covered it with a glass cover, and placed both vessels close to each other.

On the 26th of May, twenty-four hours afterwards, I examined a small drop of each by the first magnifier of Wilson's microscope, whose focal distance is reckoned at $\frac{1}{30}$ th part of an inch, and to my amazement they were both full of animalcula of a linear shape, very distinguishable, moving to and fro with great celerity; so that there appeared to be more particles of animal than vegetable life in each drop.

This experiment I have repeatedly tried, and always found it to succeed in proportion to the heat of the circumambient air, so that, even in winter, if the liquors are kept properly warm, at least in two or three days the experiment will succeed.

In Mr. Needham's experiments he calls these *spermatic animals*, Philosophical Transactions, vol. XLV. p. 644 and 666; what I have observed are infinitely smaller than real spermatic animals, and of a very different shape; the truth of which every accurate observer will soon be convinced of, whose curiosity may lead him to compare them; and I am persuaded he will find they are no way a-kin to that surprizing part of nature. And though some philo-

philosophers of great reputation have agreed in sentiment with Mr. Needham, yet I am satisfied, that whenever this subject is taken up again, and properly attended to, the world will be convinced they have been too hasty in their conclusions.

At present I shall pass over many other curious observations, which I have made on two years experiments, in order to proceed to the explaining a hint, which I received last January from Mr. De Saussure, of Geneva, when he was here; which is, that he lately found one kind of these *animalia infusoria*, that increases by dividing across into nearly two equal parts.

I had often seen this appearance, in various species, a year or two ago; as I found upon looking over the minutes I had taken when I made any new observation; but always supposed the animals in this dividing state to be in coition.

Not hearing till after Monsieur De Saussure had left this kingdom, from what infusion he had made his observation; his friend, Doctor De la Roche, of Geneva, informed me, the latter end of February last, that it was from hempseed.

I immediately procured hempseed from different seedsmen, in distant parts of the town: some of it I put into New-river water, some into distilled water, and some I put into very hard pump-water; the result was, that in proportion to the heat of the weather, or the warmth in which they were kept, there was an appearance of millions of minute animalcula in all the infusions; and some time after, some oval ones made their appearance, as at TAB. VI. Fig. 1. *b. c.* These were much larger than the first, which still continued;

nued; these wriggled to and fro in an undulatory motion, turning themselves round very quick, all the time that they moved forwards. I was very attentive to see these animals divide themselves; and at last I perceived a few of the appearance of Fig. 1. *a.*, as it is represented by the first magnifier of Wilson's microscope; but I am so well convinced by experience, that they would separate, that I did not wait to see the operation: however, as the following sketches, which I have drawn from five other species, will very fully explain this extraordinary phænomenon, there will be no difficulty in conceiving the manner of the first. See Fig. 2, 3, 4, 5, and 6.

The proportion of the number of the animals, which I have observed to divide in this manner, to the rest, is scarce 1 to 50: so that it appears rather to arise from hurts received by some few animalcula among the many, than to be the natural manner in which these kind of animals multiply: especially if we consider the infinite number of young ones which are visible to us through the transparent skins of their bodies, and even the young ones that are visible in those young ones, while in the bodies of the old ones.

But nothing more plainly shews them to be zoophytes than this circumstance; that when, by accident, the extremity of their bodies has been shrivelled for want of a supply of fresh water, the applying more fresh water has given motion to the part of the animal that was still alive; by which means this shapeless figure has continued to live and swim to and fro all the time it was supplied with fresh water.

I cannot finish this part of my remarks on these animals, without observing, that the excellent Linnaeus

næus has joined the *beroe* with the *volvœx*, one of the *animalia infusoria*. The *beroe* is a marine animal found on our coasts, of a gelatinous, transparent nature, and of an oval or spherical form, about half an inch to an inch diameter, divided like a melon into longitudinal ribs, each of which is furnished with rows of minute fins, by means of which this animal, like the *animalia infusoria*, can swim in all directions with great swiftness. In the same manner I have seen most of these minute animals, which move so swift that we could not account for it, without supposing such a provision of nature, which is really true; but cannot be seen till the animals grow faint for want of water; then, if we attend, we may, with good glasses, plainly discover them*.

I come now to a singular property, which I have discovered in hempseed, of producing an indissoluble

* I have lately found out, by meer accident, a method to make their fins appear very distinctly, especially in the larger kind of animalcula, which are common to most vegetable infusions, such as the *Terebrella*: this has a longish body, with a cavity or groove, at one end, like a gimblet: by applying there a small stalk of the horse-shoe *Geranium* (or *Geranium zonale* of Linnæus), fresh broken, to a drop of water in which these animalcula are swimming, we shall find, that they will become torpid instantly, contracting themselves into an oblong-oval shape, with their fins extended like so many bristles all round their bodies; the fins are in length about half the diameter of the middle of their bodies. Before I discovered this expedient, I tried to kill them by different kinds of salts and spirits; but though they were destroyed by this means, their fins were so contracted, that I could not distinguish them in the least. After lying in this state of torpidity for two or three minutes, if a drop of clean water is applied to them, they will recover their shape, and swim about immediately, rendering their fins again invisible.—For the different states of this animalcule, see TAB. VI. Fig. 5. *a, b, c, d.*

salt,

salt, when infused for some time in water: and as hempseed is known to be an efficacious medicine in some particular cases, these experiments may demand a stricter enquiry from the professors of physic, which may possibly turn to the benefit of mankind.

EXPERIMENT I.

On the 25th of February last, I put half an ounce of hempseed to about two ounces of New-river water in a vial, and covered it close with paper, to prevent the dust coming to it: by the 25th of March it became very putrid, and had thrown up a viscid scum to the top. Fahrenheit's thermometer in the house was, during this time, from about 44 to 52 degrees. I examined this scum with a common magnifier, of about an inch focus, and could discover it to be full of regular-shaped salts, which lay on the surface; some of a square, others of an oblong figure.

Applying some of the scum to a slip of glass, I placed it in the single or Wilson's microscope, making use of the fourth magnifier, and it exhibited the crystals represented before fig. 7; but as the stirring of the scum had obscured the precise figure of the salts, I applied a hair pencil to them, dipt in clean river water, and separated them from the mucilage that had besmeared them; yet, notwithstanding this addition of water, their figures were not in the least impaired or melted, but their outlines were rather more exactly defined. Nor were the millions of mi-

nute animals that were swimming over them, and all round them, in the least affected by the salt *.

I further observed, that the crystals that appeared first increased in size, and began to vary their forms; for instance, many of the crystals, at the latter end of April, among the rest, were of the form of those in the line of fig. 8. About the 5th of May, many of them appeared as at fig. 9; and at the latter end of May, about the 20th, many of them were of the form of those at fig. 10: most of the variety of forms appearing at the same time.

It was objected by some very ingenious men, to whom I had imparted this discovery, that these salts might be owing to something in the water that I had made use of, which, joined to the oil in the hempseed, might produce this appearance. To obviate this:

EXPERIMENT II.

I prevailed on my friend, Mr. P. Woulfe, F. R. S. to furnish me with some water that had been most carefully distilled, by a very slow process; and at the same time I procured hempseed from a different part of the town. On the 30th of April, I put an ounce of this hempseed to about four ounces of this distilled water, into a glass cylindrical vessel, and covered it carefully with a glass cover; and on the 12th of May I examined the scum, and found it more transparent,

* Mr. Needham observes, in his curious Memoir before mentioned, p. 649, that salt destroys these animalcula; this, I believe, is very true of the common kinds of salt; and which renders the nature of this kind of salt still more singular.

but

but full of the crystals of salts, as represented at fig. 12. Some of the first hempseed put into the same water produced much salt, but not so regular in its figures; these figures, by some means unknown to me, after their crystallization being broke irregularly at their ends, see fig. 13. But yet in this infusion there were many of the original feminal figured salts.

EXPERIMENT III.

I was determined to see what effect the hard pump water of Gray's-Inn, after a month's dry weather, would have on the hempseed in infusion; particularly as I was persuaded from experience, that this water contained a large portion of calcareous earth. Accordingly, on the 5th of May, I put an ounce of the same hempseed with the last which I had obtained, into four ounces of this pump water; and on the 17th of May I perceived the crystals, which, on being put into the microscope, with the same magnifier, gave the appearance represented at fig. 14.

The crystals of this infusion seemed larger and flatter, and something different in their shape; but on examining the mucilage that lay among the seeds at the bottom of the glass, I found an infinite number of the same shaped crystals with those I have called feminal crystals; which were likewise found in the mucilage of the New-river water infusion, and in the distilled water infusion among the seeds.

I must further observe, that the calcareous earth floated in great abundance among the scum of the pump-water, as soon as the putrefaction was advanced;

vanced; which did not appear on the surface of the distilled water, and scarcely any on the river water.

The grains of salt produced in these experiments were about the size of the finest basket salt, and of a pale yellowish colour when dry.

Gray's-Inn,
May 24, 1769.

P O S T S C R I P T.

I have since found the same kind of crystals in an infusion of flax-seed in New-river water, and also in wheat that has been infused in boiling hot water; but the crystals were fewer, and did not appear so soon in the flax-seed as in the hemp-seed. And the experiment of wheat infused in boiling hot water does not always succeed.

I have likewise found salts not unlike those of the hempseed, in infusions of a variety of pulse and grain from the East Indies, such as lupines, kidney-beans, vetches, millet, Guinea-corn, and the sesamum or oily grain: but the last yielded a much larger quantity of salt, and in a shorter time, than any of the rest.

The salts of these different substances were also not dissolvable upon applying clean water to them; but by letting the infusions continue to putrefy some weeks longer, they by degrees assumed irregular shapes, and disappeared. I must conclude, then, with this quere, Are not these the oily parts of the vegetables, which float in the scum, on the surface of the infusion, crystallized?

Explanation of the FIGURES, TAB. VI.

These five different kinds of *animalia infusoria*, belonging to the genus of *Volvox* of Linnæus, are here represented both in their perfect and in their divided state. The trivial names are added to distinguish the species.

Fig. 1. represents the *volvox ovalis*, or egg-shaped volvox : at (c) and (b) it is expressed in its natural shape : at (a) the manner in which it becomes two animals, by separating across the middle : this was found in the infusion of hempseed, but is found in other vegetable infusions, particularly in that of tea-feed.

Fig. 2. is the *volvox torquilla*, or wryneck. At (a) is represented its divided state, at (b) and (c) its natural shape; this is common to most vegetable infusions, as is the following.

Fig. 3. is the *volvox volutans*, or the roller. At (a) the animal is separated, and becomes two distinct beings, each swimming about and providing for itself; this is often the prey of another species of this genus, especially while it is weak by this separation, not being so active for some time till it can recover itself. At (c) the animal appears to be hurt on one side; this impression, in a little time, is succeeded by another on the opposite side, as at (b), which soon occasions a division.

At

At (*d*) is the side view, and at (*e*) the front view of the natural shape of the animal.

Fig. 4. is the *volvox oniscus*, or wood-louse.

At (*a*) is the natural shape of it, as it appears full of little hairs both at the head and tail; with those at the head it whirls the water about, to draw its prey to it; the feet, which are many, are very visible, but remarkably so in a side view at (*d*). At (*b*) it is represented beginning to divide, and at (*c*) the animals are ready to part: in this state, as if in exquisite pain, they swim round and round, and to and fro, with uncommon velocity, violently agitated till they get asunder. This was found in an infusion of different kinds of pine branches.

Fig. 5. is the *volvox terebrella*, or the gimblet.

This animal is one of the largest of the kind, and is very visible to the naked eye. It moves along swiftly, turning itself round as it swims, just as if boring its way. (*a*) and (*b*) are two views of its natural shape. (*c*) shews the manner of its dividing. When they are separated, the lower animal rolls very awkwardly along till it gets a groove in in the upper part. (*d*) represents one of them lying torpid, by means of the juice of the *horseshoe geranium*, with its fins extended. This animal is found in many infusions, particularly of grass or corn.

Fig. 6. is the *volvox vorax*, or the glutton.

This animal was found in an infusion of the Tartarian pine; it varies its shape very much, contracting

Contracting and extending its proboscis, turning it to and fro, in various directions, as at *a, b, c, d, e*. It opens its proboscis underneath the extremity, when it seizes its prey. The less active animals, that have lately been divided, such as those at Fig. 2. *a*. and Fig. 3. *a*, serve it as food when they come in its way: these it swallows down instantly, as it is represented at Fig. 6. *b*. and *i*. At (*f*) it is ready to divide, and at (*g*) it is divided, where the hinder part of the divided animal has got a proboscis or beak, to procure nourishment for itself, and soon becomes a distinct being from the fore-part.

Fig. 7. represents the appearance of the salts in hempseed, after a month's infusion, from the 25th of February to the 25th of March, in New-river water.

Fig. 8. The salts, about a month after, April 25, appeared in this manner.

Fig. 9. These figures represent them about the 5th of May, or ten days after.

Fig. 10. About the 20th of May, they exhibited the figure of precious stones.

Fig. 11. These I have called seminal salts, as these small figures are to be seen in most of the infusions, rising at different times, and exhibiting these shapes, when they first appear distinctly,

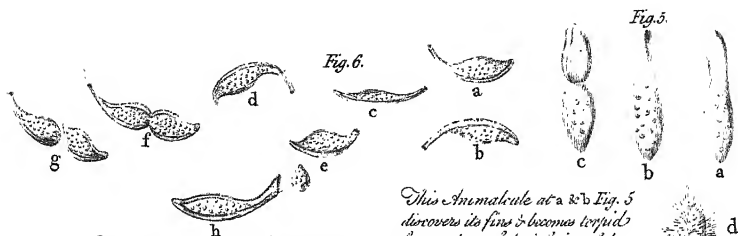
Fig. 12. represents the salts of hempseed in distilled water, that had been infused from the 30th of April to the 12th of May.

Fig.

Fig. 13. shews the form of the salts when the putrefaction had begun to separate their parts into laminæ in the distilled water.

Fig. 14. are the figures of the salts that appeared from the hempseed, infused in hard pump-water about twelve days, from the 5th of May to the 17th.

The Figures of 6 kinds of Animalcula infusoria, that increase by dividing across the middle into two distinct animals.

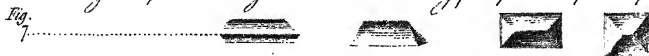


This Animal opens its Throat thus & swallows many of those animalcula that have been just divided.

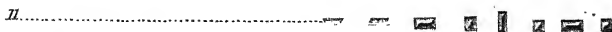
This Animalcule at a & b Fig. 5 discovers its fins & becomes torpid from a drop of the infusion of the Hemus shoe Geranium



Figures of Salts, not dissolvable in Water, arising from putrid infusion of Hemus shoe.



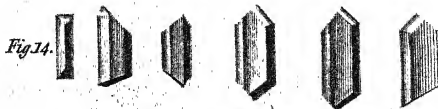
In New River Water



Salts decaying



In distilled Water



In hard pump Water

XIX. *On the Computation of the Sun's Distance from the Earth, by the Theory of Gravity: In a Letter to Mathew Maty, M. D. Sec. R. S. from the Rev. Mr. Horley, F. R. S.*

S I R,

Read June 1,
1769. **A** LITTLE Treatise, that has lately been published, against Dr. Stewart's method of determining the distance of the Sun by the theory of gravity, has put me upon reconsidering a subject which I had long dismissed from my thoughts. I am far from being convinced that Dr. Stewart's conclusions are "erroneous upon his own principles," as his antagonist affirms; and I am well satisfied that there is no error in the principles themselves. I have always been sensible that an extreme precision was requisite in determining the mean quantity of the solar force affecting the moon's gravity towards the earth, in order to obtain an accurate estimation of the distance; and this circumstance was mentioned by me, in a paper that I communicated to the Society about two years ago*, before it had been remarked, that I knew of, by any other writer upon the subject. I must now declare, that the imperfection of the method arising from this

* See Philos. Transf. vol. LVII. for 1767, p. 179. 183.

circumstance is much greater than I was at first aware of. I owe this better information entirely to the revival of Dr. Stewart's Theorems, not to any thing that has been written upon the subject by others. I find that if I increase the mean quantity of the sun's disturbing force, as determined by Dr. Stewart in the 9th proposition of his Supplemental Tract, and by myself, in my former paper, by $\frac{1}{250000}$ th part of itself, I obtain, by my own method of computation, $9''\ 3''$, 394 for the Sun's mean horizontal parallax; which seems to be so nearly the mean of the quantities of the parallax deduced from the best observations of the transit of 1761, that it would be ridiculous to set up, any longer, the conclusions of this theory in opposition to observation. It is much more probable that the theory should err in so small a matter as $\frac{1}{250000}$ th of the Sun's disturbing force, than that observation should err in more than $\frac{2}{3}$, that is nearly in $\frac{1}{4}$ of the whole quantity in question. I beg the favour of you to communicate this to the Society.

I have the honour to be, Sir,

Your most obedient,

and most humble servant,

Oxford, May 5,
1769.

Samuel Horsley.

XX. *Meteorological Observations for 1768, made at Bridgwater in Somersetshire, and at Ludgvan in Mount's-Bay, Cornwall. Communicated by Dr. Jeremiah Milles, Dean of Exeter, and F.R.S.*

Read June 1, 1769.

OBSERVATIONS at BRIDGWATER, 1768.

1768. Month.	Barometer.	State of the Weather and Wind.	Fahrenheit's Thermom.	Onbr.
January	In. 3. Hight 30, 14 24. Lowest 29, 18	From the 1st to the 8th, frosty and remarkably cold. On the 9th a very deep snow, 1 foot 3 inches undriven; and it would have been deeper, had it not rained the day before. From thence, to the end of the month, cloudy, foggy, or rainy.—The wind northerly the first part, and in the latter variable from thence to N. E.	Med. 25. Hight 48 } 4. Lowest 9 }	In. 3, 905
February	5. Hight 30, 42 10. Lowest 29, 27	Changeable throughout; either cloudy or rainy, with very little sun-shine. The wind either N. E. or N. W. the first part of the month, and generally S. W. the remainder. Very high in the middle of the month.	11. Hight 53 } 3. Lowest 28 }	2, 390
March	4. Hight 30, 46 15. Lowest 29, 73	The first part generally cloudy, the middle sun-shine, and the latter part cloudy. The wind either N. E. or N. W. almost the whole month, and remarkably calm, except two or three days in the middle.	29. Hight 55 } 4. Lowest 28 }	1, 25
April	1. Hight 30, 21 29. Lowest 29, 50	Constant sun-shine for the first five days, but from thence to the end of the month generally stormy. The wind chiefly westerly the whole month.	14. Hight 59 } 3. Lowest 39 }	2, 829 May

Month.	Barometer.	State of the Weather and Wind.	Fahrenheit's Thermom.	Ombr.
May	In. 15. Highest 30.22 29. Lowest 29.24	Stormy for a few days in the beginning, but generally sunshine through the remainder. The wind chiefly northerly. N. B. A very remarkably strong Aurora Borealis on the 4th, and very uncommon, almost constant, lightning for several hours, on the 5th, but without injury.	Med. 23. Highest 74 } 1. Lowest 40 }	In. 1,270
June	2. Highest 30.15 9. Lowest 29.26	Sun-shine for the first seven days; from thence stormy, cloudy, and sun-shine, intermixed. The wind variable, between N. W. and S. W.	6. Highest 69 } 19. Lowest 48 }	3,935
July	21. Highest 30.05 7. Lowest 29.43	Generally rainy, and chiefly towards the latter end. The wind generally S. W.	28. Highest 73 } 9. Lowest 50 }	4,565
August	8. Highest 30.12 23. Lowest 29.43	Changeable throughout. Sun-shine, cloudy, and rain. The wind most commonly northerly.	14. Highest 69 } 29. Lowest 52 }	2,485
September	14. Highest 30.12 16. Lowest 28.94	The first and last weeks cloudy and sun-shine intermixed. But almost constant rain during the middle of the month. The wind generally S. W.	27. Highest 65 } 14. Lowest 46 }	6,207
October	19. Highest 30.14 5. Lowest 29.14	The first part generally cloudy, with hard storms. The middle cloudy and sun-shine intermixed; and the latter part stormy. The wind chiefly westerly.	7. Highest 63 } 22. Lowest 40 }	4,015
November	6. Highest 30.33 22. Lowest 28.23	Changeable throughout. The wind generally either N. W. or S. W. and high. The barometer was remarkably low the latter part of this month.	8. Highest 59 } 9. Lowest 36 }	5,595
December	12. Highest 30.43 2. Lowest 28.89	The first part changeable; smart frost in the middle, and generally cloudy in the latter part. The wind generally S. W.	8. Highest 56 } 15. Lowest 22 }	2,170

Total of Rain

39.291

O B S E R-

OBSERVATIONS at LUDGVAN in MOUNT'S-BAY, CORNWALL, 1768.
By WILLIAM BORLASE, D. D.

1768. Month.	Barometer.	State of the Weather and Wind.	Fahrenheit's Thermom.	Ombr.
January	3. Highest 29,99 1. Lowest 28,88	On the 1st stormy, thunder, lightning. Hard frost, with some snow, from the 2d to the 6th. The 6th a. m. snow gone, but the ice in a garden watering-pot found ten inches deep. The 7th thaw with much rain. 8th a. m. frost all gone. On the 9th melting snow and rain. No more snow or frost. Rest mostly stormy with rain. Wind chiefly East and North.	Med. In. 25. Highest 49 } 3. Lowest 25 } 40 $\frac{1}{2}$ 6,900	
February	6. Highest 30,29 10. Lowest 29,10	Cloudy. 10th a violent storm, wind S. by W.; extreme at noon. The rest mifty, stormy, showery, and rain. Little sun-shine or calm. Wind blew twenty days from the West.	21. Highest 54 } 3. Lowest 34 $\frac{1}{2}$ } 47 $\frac{1}{2}$ 3,760	
March	11. Highest 30,27 5. Lowest 29,69	Gentle frosts, for a day only, on the 4th, 8th, and 24th. Rest mostly fair and sun shine. Barometer unusually high. Wind chiefly from the East and North.	17. Highest 55 } 4. Lowest 33 } 44 $\frac{7}{8}$ 0,350	
April	1. Highest 30,3 29. Lowest 29,10	Weather windy, with frequent showers. The wind in the first third of the month East, and East-North-East, rest Westerly, mixed with the South.	18. Highest 56 } 1. Lowest 41 } 49 $\frac{3}{8}$ 3,180	May

Month.	Barometer.	State of the Weather and Wind.	Fahrenheit's Thermom.	Omb.
	In.		Med.	In.
May	23. Hightest 30.5 29. Lowest 29.10	Much calms and sun-shine. Wind mostly from the East, mixed with the North. Pleasant month.	25. Hightest 70 2. Lowest 43	1,550
June	2. Hightest 30.4 11. Lowest 29.10	On the 6th thunder, lightning, and rain. The 25th ditto. On the 11th stormy. 27th ditto. Rest fair, sun-shine and rain intermixed. Wind equally from all points.	3. Hightest 66½ 13. Lowest 49	4,750
July	21. Hightest 29.90 7. Lowest 29.18	Showery, rainy, and fair, intermixed. Windy and calm nearly equal. Stormy rain on the 26th. Wind blew twenty-six days from the West, mostly mixed with a point of the South.	30. Hightest 66 17. Lowest 53	4,900
August	8. Hightest 30.3 22. Lowest 29.31	To the 16th fair, generally, with sun-shine, and much calms. On the 17th p. m. thunder, lightning, and violent rain, of which at least one inch fell from 2 p. m. to 5 p. m. N. E. Great falls of rain on the same day in Yorkshire, Wales, &c. Wind then at East and North. Rest mostly from the West.	16. Hightest 67½ 2. Lowest 55	3,900
September	14. Hightest 30.6 16. Lowest 28.97	Beginning showery, calm, and sun-shine. On the 12th violent rain and wind. 13th, 22d, ditto. On the 13th, 17th, 22d, 25th, 26th, stormy. On the 16th a storm, Wind W. E. S. One half of the month wind from the Westward; the rest mixed.	28. Hightest 64 21. Lowest 47	4,510
October	19. Hightest 30.2 5. Lowest 28.59	Rainy, windy month. On the 5th and 6th stormy rain. 7th, 18th, 29th, ditto. On the 6th thunder, lightning, and hail. One half of the month wind blew from the West; the rest mixed.	1. Hightest 61 13. Lowest 45	5,010

Month.	Barometer.	State of the Weather and Wind.	Fahrenheit's Thermom.	O nbr.
November	<p>6. Highest 30.17 22. Lowest 28.23</p> <p>In, In, 30.17</p>	<p>On the 4th violent hail-showers. On the 2d hard rain. 10th, 11th, 21st, 26th, 27th, 28th, 29th, ditto. On the 4th, 16th, 21st, 22d, 23d, 24th, 28th, 29th, stormy. Wind blew twenty-seven days from the West. N. E. From Sunday the 20th 8½ p. m. to Tuesday 22 half hour p. m. (viz. in 39½ hours) the mercury in the barometer fell 175 (all decimals), and at half past noon was down to 28.23, the lowest yet noted here. Wind W. ½ N. By 8½ p. m. of the 22d, the mercury was risen to 28.31.</p>	<p>Med. In.</p> <p>1. Highest 54 } 22. Lowest 41 } 48.5 48.5 46.3</p>	<p>6,930</p>
December	<p>17. Highest 30.17 2. Lowest 28.70</p>	<p>Violent rains. From December 1, 10 a. m. to December 2 noon, in 26 hours the fall of rain was 1 inch 2.50. On the 6th lightning, and stormy showers. On the 16th a storm, wind S. E. On the 13th smart frost. 14th ditto. 15th a deep fall of snow; but that, and the frost, diminished by the next morning, by stormy, violent rains, about 1 inch having fallen on the 15th at night. Wind mostly from the South and West.</p>	<p>Highest 52 } Lowest 40 } 46.8 46.8 46.3</p>	<p>5,150</p>

Rain fallen in the Year 1768 52,890

XXI. *Proposal of a Method for securing the Cathedral of St. Paul's from Damage by Lightning; in consequence of a Letter from the Dean and Chapter of St. Paul's to James West, Esquire, Pr. R. S.*

March 9, 1769,

The following Letter to the President was read; videlicet,

St. Paul's, March 6, 1769.

S I R,

THE consideration of the old church of St. Paul's having twice suffered by lightning, and a solicitude to secure the present fabric from similar accidents; which, but for the interception of the storm by St. Bride's church, within these few years, might have already happened; induce us, the Dean and Chapter of this cathedral, to request the opinion of the Royal Society (so justly eminent for the abilities of its members in every branch of science), relative to the best and most effectual method of fixing electrical conductors. We shall esteem ourselves obliged to the very respectable body over which you preside,

preside, for their sentiments and directions on this subject, and are, with much regard,

S I R,

Your most obedient,

humble servants,

Thomas Bristol, D.

Chr. Wilfon.

S. Barrington.

J. Lich. & Cov.

To James West, Esquire, President of the Royal Society.

In consequence of this application, it was desired that John Canton, M. A. Edward Delaval, Esquire, Benjamin Franklin, LL.D. William Watfon, M. D. and Mr. Benjamin Wilfon, be a Committee to consider the above letter, and report their opinion thereon to the Society; and, accordingly, June 8, 1769, Dr. Watfon, at the meeting of the Society, read, in his place, a report from the Committee appointed to consider the application from the Dean and Chapter of St. Paul's, relating to the preservation of that elegant structure from damage by lightning; for which report, thanks were ordered to the Committee, and returned to Dr. Watfon: and it was also ordered, that a copy of the said report be transmitted to the Dean and Chapter of St. Paul's, signed by the Secretary.

Report from the Committee appointed to consider of the properest means to secure the Cathedral of St. Paul's from the Effects of Lightning. Addressed to James West, Esquire, President of the Royal Society.

S I R,

Read June 8, 1769. **A**S, in consequence of a letter addressed to the Royal Society from the Dean and Chapter of St. Paul's, the Society did us the honour to appoint us a Committee to examine that magnificent structure, and, as far as our experience would enable us, to prevent mischief thereto from lightning, by a properly disposed apparatus; we lay before you the following as our opinion thereupon, to be communicated, if you think proper, to the Royal Society. And here, Sir, you will permit us to take notice of, and acknowledge, the obligations we were under to Mr. Mylne, a very worthy member of this Society, who is surveyor of St. Paul's, and attended several meetings of the Committee. This gentleman furnished us with a great variety of information, in relation to the structure of the several parts of this fabric, which, without his assistance, could not easily have been obtained.

As all metals are now known readily to conduct or transmit the electric fluid, or, which is the same thing, lightning, through them; the large quantity of lead, and some iron, disposed in different parts of St. Paul's church, will, by having its several parts
connected,

connected, where there is at present no such connection, prevent the erecting a considerable part of the apparatus, which otherwise we should judge absolutely necessary.

We are of opinion that, *cæteris paribus*, all buildings upon the same level are liable to be injured by lightning in proportion to their height: and that the danger is increased by crosses, weather-cocks, or pieces of metal, in any form, placed upon or near their tops, unless there is a compleat metallic communication from these to the bottom of the building, which metal should terminate either in water, or moist ground.

In St. Paul's church, the objects of our more particular attention were the dome and its lantern, and the two towers at the west end. The roof over the body of the church, being compleatly covered with lead, will, we conceive, prevent mischief thereto from lightning; and the more so, as the lead on the roof joins to that of the several leaden spouts, which come down the sides of the building, and terminate in the ground at a considerable depth. For our more certain information, one of these spouts was examined; and it was found to descend perpendicularly about three feet under the surface of the earth: and then, after being laid about seven feet in an inclined direction, it ended in a brick drain, which communicates with the sewer. These circumstances induce us to conclude, that what has been just now described is a sufficient metallic communication between the roof of the church and the ground.

No part of this whole fabric seems to be in so dangerous a situation of being injured by lightning, as

the stone lantern placed above the dome. This danger arises not only from its height, but from the different pieces of metal in different parts of it, being at present detached and separated from each other. This stone lantern is supported by a truncated cone of brick-work, of no more than eighteen inches, or two bricks, thick. To the honour, however, of the architectural sagacity of Sir Christopher Wren, who was formerly our President, this support of the lantern, which has already stood much above half a century, has not in the least given way in any of its parts. How far it would sustain the violence of a stroke of lightning will, it is to be hoped, never be tried: and what we have now to propose will, we flatter ourselves, lessen the probability of its being injured by it. The first object of our attention, therefore, was to make a compleat metallic communication between the cross, placed over this lantern, and the leaden covering of the great dome; as from its height, if any lightning was in its neighbourhood, it would most probably affect the cross.

This cross with the ball, both composed of metal, are supported by, and connected with, seven iron rods. These descend perpendicularly through the small leaden dome, which covers the lantern, and are inserted into and pass through a strong frame of timber, placed horizontally under that dome. The lower extremities of these iron rods are fastened to the under surface of this timber frame with iron nuts and screws.

From this timber work, several large iron bars, placed at some distance from the ends of the above-mentioned iron rods, descend obliquely, and are fixed
in

in the stone-work of the lantern. The upper ends of each of these oblique iron bars pass through the frame of timber before mentioned, and are fastened to its upper surface with iron nuts and screws. Between these iron bars and the leaden covering of the great dome, there is at present no metallic communication. To this arrangement, therefore, is owing the danger from lightning, which the Committee apprehends that this part of the building is liable to. To obviate which, we are of opinion, that four additional iron bars, each not less than an inch square, should be securely placed over the frame of timber before mentioned in such a manner, that one end of each of these four additional iron bars may be in contact with one of the perpendicular iron rods, and the other end of each be in contact with one of the iron nuts and screws, which fasten the obliquely descending iron bars to this frame of timber. At the bottom of these oblique iron bars, just above where they are inserted into the stone-work, the Committee recommends, that a ring, made of bar iron, of about an inch square, should be placed so as to be fastened to, and be in contact with, these iron bars.

From this proposed ring to the upper part of the lead which covers the great cupola, the distance is about forty-eight feet. In this space, we are of opinion, that four iron bars should be placed, each not less than an inch square. These should be fixed within the lantern in such a manner, that the upper end of each should be fastened to, and in contact with, the iron ring before mentioned, and their lower ends in contact with the lead on the upper part of the cupola; from which the metallic communication is complete.

completed to the lower end of the pipes, that discharge the water from the circular part of the great cupola, upon the floor of the stone gallery.

From the bottoms of these pipes, which terminate with a shoe of lead within half a foot of the floor of the stone gallery, the metallic communication is again interrupted to the top of the leaden pipes, which convey the water from thence. Here it is proposed, that conductors of lead, not less than four inches in breadth and half an inch in thickness, should be placed so as to be in contact with the bottom of four of the pipes that come from above, and with the top of four of those that descend. Lead is recommended to be employed here, as more readily adapting itself to the various curvatures it must meet with in the now proposed arrangement.

These last pipes, after descending below the colonnade, near the circular stair-cases, make their appearance upon the outside of the drum-part of the cupola ; where they are bent at obtuse angles, and discharge their water upon the roof of the church. From these angles to the roof the distance is about five feet. Here then is another interruption to the metallic communication. This is proposed to be completed by conductors of lead, similar to those before mentioned, which should be so placed as to be in contact both with the bottom of the pipes and the adjoining roof.

From the roof, as has already been mentioned, the leaden pipes are continued below the surface of the earth, and terminate in a drain ; and thus, by the method now directed, the metallic communication will be completed from the cross on the top of

St. Paul's church to some feet below the surface of the ground.

The Committee then turned their thoughts towards the two towers at the west end of the church ; and here they beg leave to observe, that in one of these towers, between the pine apple and the leaden bell-shaped covering near it, placed at the top of each of these towers, there is no metallic communication deserving notice, till you come to the lead on the roof of the church. This distance is eighty-eight feet. To this tower, therefore, it is proposed to adapt a rod or bar of iron, not less than an inch and a quarter square, in such a manner that one end of the bar should be in contact with the metal communicating with the pine apple on its top, which is of copper, and the other end with the lead on the roof of the church.

In the middle of the other tower, in which the great bell is hung, there is an iron stair-case of considerable height, which is placed in the middle of it, in order for the more conveniently coming at the clock-work. The top of this stair-case is at no great distance from the leaden covering upon the top of the tower : but from the bottom of this stair-case to the roof of the church, between which there is no metallic communication, the distance is considerable, not less than forty feet. The Committee recommend, therefore, that a bar of iron, of an inch and a quarter square, may be placed between the pine-apple; or the lead in contact with it, and the upper part of this stair-case ; and that another iron bar, similar to this last, may be adjusted so, as to pass from the bottom of the stair-case to the lead on the roof of the church. The roof,

as has been already mentioned, communicates with the leaden pipes, and these with the ground.

These towers, from their near situation to the cupola, which is a building so much higher, may possibly be less liable to mischiefs from lightning than if they were erected at a more considerable distance. As the direction of the lightning is, however, uncertain, from a variety of causes, as also to what extent one building will protect another, the Committee are of opinion, that this apparatus to the towers will be expedient.

It is to be remarked, that wherever iron is employed as a conductor of lightning, especial care must be taken to prevent its becoming rusty; as, from being long exposed to the moist atmosphere, it will be corroded to a considerable depth: and so much of the iron as is corroded ceases to be of use as a conductor; the Committee therefore have, in directing the size of these iron bars, made some allowance for the waste of the iron by rust.

The size, as well as number, of the iron bars recommended here by the Committee, are only to be considered as applicable to St. Paul's, and not as a standard for any church or building of less dimensions; as in these last, conductors of a smaller size, and fewer in number, may answer the purpose as securely as the larger. But St. Paul's church is particularly circumstanced: it is an edifice not only of great height, but its cupola, to say nothing of the lead on the body of the church, presents a large surface of metal to the clouds; on which account it is very liable to receive greater quantities of the electric fluid; and, from large quantities of such an elastic power,

great

great mischiefs may arise to this magnificent building, in consequence of obstructions the fluid may meet with in passing through it. For these reasons we have recommended very large conductors, that it may pass through them into the ground, as readily as it enters.

These, Sir, are our sentiments in relation to the matter, referred to us by the Royal Society, upon the request of the Dean and Chapter of St. Paul's. If they should be acceptable to the Society, and by their means to the Dean and Chapter; and if, by being carried into execution, they should at all contribute to the preservation of that noble fabric, it will be a great satisfaction to us. We are, with very great respect,

SIR,

Your most obedient,

humble servants,

W. Watfon.

B. Franklin.

B. Wilfon.

John Canton.

Edward Delaval.

7 June, 1769.

XXII. *Observation of the late Transit of Venus: In a Letter to James Burrow, Esquire, V. P. R. S. By Mr. James Horsfall, F. R. S.*

S I R,

Read June 8, 1769. **L**OOKING upon it as the duty of every member of the Royal Society, to contribute their mite towards the discovery of such an important matter as *The dimensions of the solar system*, I therefore take the liberty to communicate to you the following observations, made upon the late transit of Venus.

They were made with a Gregorian telescope, magnifying at least 100 times.

The time undermentioned is equal time.

The rate of going of the two clocks had been correctly ascertained by my friend Dr. Bevis, and myself, by observing the Sun pass the meridian for several days preceding the day of the transit.

My situation was upon a platform laid upon the ridges of my own chambers near the Middle Temple Hall: consequently, the great volumes of smoke arising from the houses to the north-west were no small impediment to a good observation: this was very remarkable at the *time of internal contact*. The Sun's limb undulated then prodigiously, and there was also a gust of wind which made the telescope vibrate, but
not

not so much as once to lose the planet out of the field. I pronounced that moment (I mean that of internal contact) as soon as I saw a lambent light (*not a well-defined light*) whirl round the opaque limb of the Planet; from whence I am inclined to think I pronounced that *too early* by two or three seconds.

The first part of this rare phenomenon, which I beheld, was a kind of penumbra, at $7^h 8' 50''$.

I then counted 1, 2, 3 to $8''$, and plainly discerned the dark limb of Venus make a dent very near the vertex of the Sun's limb.

At $7^h 26' 34''$ I perceived the lambent light above-mentioned; at which time the Sun was not above $\frac{1}{4}$ of a degree above the top of a chimney.

This is a simple and faithful relation of what I observed, which I had not even copied till this morning, after you was pleased to notify your expectation.

I am glad of this opportunity publicly to acknowledge the many obligations you have been pleased to confer on me; and am,

S I R,

(with great respect),

Your much obliged,

and most obedient,

humble servant,

Middle-Temple,
8 June, 1769.

J. Horsfall.

XXIII. *An Account of the Observations of the Transit of Venus and of the Eclipse of the Sun, made at Shirburn Castle and at Oxford. By the Reverend Thomas Hornsby, M. A. F. R. S. and Savilian Professor of Astronomy in the University of Oxford.*

Read June 15, 1769. **T**HE weather, on the morning of the 3d of June, was so very unfavourable, both at the observatory of the Earl of Macclesfield and also here at Oxford, that there was very little reason to expect that we should be able to make any observation. But here, a few minutes before noon, the clouds began to break, and I was enabled to observe the transit of the Sun's consequent limb over the meridian. At one o'clock in the afternoon, the sky was again overcast, and it rained for some time; but towards three o'clock, the clouds were dispersed, the Sun shone out clearly, and at five o'clock there was hardly a cloud to be seen. The preceding evening was also so very favourable, that the several persons who proposed to make observations of the transit, had an opportunity of adjusting their instruments.

The Right Honourable the Earl of Macclesfield made use of an excellent refracting telescope of $3\frac{1}{2}$ feet,

feet, made by Mr. Dollond, with a treble object-glass, magnifying 150 times; and at $7^h 7' 49''\frac{1}{2}$ apparent time, was certain that the planet had sensibly advanced upon the Sun's disk, having seen a small impression upon the zenith part of the Sun's limb near a minute sooner. At $7^h 23' 13''$ mean time, or $7^h 25' 28''\frac{3}{4}$ apparent time (as reduced from sidereal time), his Lordship determined the internal contact, which he judged to happen when the dark penumbra, which was so sensibly perceived between the limbs of the Sun and Planet, was lost upon the completion of the thread of light. His Lordship observed at a small distance from the observatory, by means of a stop-watch, which was let go at the instant he judged the total ingress to happen, and immediately compared with the observatory clock.

Mr. Bartlett, a very excellent observer, who has been constantly employed in the observatory for many years, observed with a 14 feet refractor on the north-side of the observatory, within hearing of the clock, the seconds of which were counted by Mr. Phelps, the other assistant observer. At $7^h 7' 4''$ apparent time, Mr. Bartlett first saw Venus upon the Sun; and at $7^h 23' 10''\frac{1}{2}$ mean time, or $7^h 25' 26''$ apparent time, he judged the ingress to happen, the telescope magnifying near 60 times.

Lady Macclesfield was also pleased to attend to the observation; and at $7^h 25' 16''\frac{1}{2}$ apparent time, judged the second internal contact to happen, with a refracting telescope of 6 feet, through which the penumbra before mentioned was hardly to be distinguished.

The

The sky, though free from clouds, was charged with vapour, which occasioned a constant undulation of the limbs of the Sun and Planet; and the wind sometimes blew so hard as to incommode the observers.

On the next morning the sky was very favourable to observation, and Mr. Phelps determined the eclipse of the Sun to begin at $18^h 32' 45'',7$ mean time, or $18^h 34' 56'',7$ apparent time, and to end at $20^h 17' 23'',5$ mean time, or $20^h 19' 33'',8$ apparent time. The Earl of Macclesfield observed the end to happen one second later, making use of Mr. Dollond's refractor.

The latitude of the observatory at Shirburn Castle is $51^{\circ} 39' 22''$, as determined by observations of the Pole Star, at several different times; and is $3' 57''$ of time west of Greenwich, and $1' 6''$ to the east of Oxford, as appears by computing the difference of meridians between Mr. Short's house, Shirburn Castle, and Oxford, as they result from the observations of the Sun's eclipse on April 1, 1764.

I proposed to observe the transit of Venus and the Sun's eclipse in the upper room of the tower of the Schools, which, though the floor of it be very unsteady, yet from its elevated situation afforded me the clearest view of the north-west part of the horizon, and is indeed the best place for making occasional observations in different parts of the heavens, and at different altitudes, which this place at present affords. The clock, furnished with a compound pendulum, was for some time carefully compared with another
clock

clock of the same construction, which is fixed in a small observatory in the house where I live, and which I had altered from sidereal to mean solar time, for the easier comparison of those clocks, which several gentlemen had procured, in order to observe this rare and curious phenomenon. The time was determined by meridional transits of the Sun, taken with a transit instrument made by Mr. Bird, and placed very exactly in the plane of the meridian, the focal length of the object-glass being 43 inches. The motion of both clocks was perfectly even and regular.

The atmosphere was so loaded with vapour, and the limb of the Sun was in such a constant state of undulation, that I determined to observe the external contact with a refractor of 12 feet, furnished with a system of eye-glasses, and magnifying 68 times. I had found, by a previous computation, that the Planet would make the first impression upon the Sun's upper limb, about nineteen minutes of a degree to the right hand of a vertical circle passing through the Sun's center. I therefore kept my eye constantly fixed upon that part, and at $7^h 5' 58''$ apparent time; I perceived that a small part of the Planet's diameter had certainly entered upon the Sun's disk; the impression, which I had observed for a few seconds before, having continued upon that part. While the Planet was passing over the Sun's edge, I determined, with the old micrometer applied to the 12 feet glass, the following differences of declination between the northern limb of the Sun, and the southern limb of Venus, with as much accuracy as the unsteadiness of the floor would permit.

Mean:

Mean Time.

	h	'	"		
At	7	8	48	3	35,2
	7	11	37	3	44,5
	7	13	28	3	46,4
	7	14	57	3	50,1
	7	15	42	3	53,6

But as the time of the internal contact began to draw nigh, I directed a refractor of $7\frac{1}{2}$ feet, with a double object-glass, to the Sun, made by Mr. Dollond, and magnifying 90 times; and soon after $7^h 21'$ mean time, perceived that the Planet appeared to be wholly entered upon the Sun, though the limbs of the Sun and Venus were not actually separated; that part of the Sun's edge, where the ingress happened, being very sensibly obscured by a penumbra, and the limbs appearing to be united, by a kind of ligament of a considerable breadth. This ligament became narrower and narrower, and was at length reduced to a point, and actually broken at $7^h 21' 57''\frac{1}{2}$ mean time, or $7^h 24' 13''\frac{3}{4}$ apparent time. At $7^h 24' 23''$ apparent time, the thread of light between the edges of the Sun and Venus, which was before compleated, now appeared to me of a very sensible breadth, and to equal $\frac{1}{10}$ th of the Planet's diameter. If I have estimated this breadth properly, the true internal contact must have happened considerably more than a minute sooner. The Swedish astronomers have described this appearance very nearly as I saw it; but according to the ac-

count * given by Mr. Mallet, the interval of time between the true and apparent ingress, when the limbs appeared perfectly to coincide, and when the ligament was observed to be broken, did not exceed $53''$, according to Mr. Melander's observation, and amounted to $56''$, according to Mr. Wargentín. This appearance, in all probability, is occasioned by the refraction which the rays of the Sun suffered in passing through the high and dense atmosphere of the Planet, and was perhaps rendered more sensible by the vapours near the horizon; as a similar appearance was observed at the second internal contact, in 1761, at very considerable altitudes, though in a smaller degree. But it will, I fear, occasion a much greater uncertainty in the quantity of the Sun's parallax deducible from these observations, than was reasonably expected.

By a mean of six observations, I found the Planet's diameter $= 58''.1$; being not greater than $59''.0$ from four of the observations, all agreeing precisely to the same part of a second; nor less than $56''.9$ by the least of the other two.

About fifteen minutes after the internal contact, a very thick and black cloud, which moved towards the east, with a slow motion, along the skirts of the horizon, prevented any further observations.

The next morning, the sky being perfectly clear, and the limb of the Sun undulating but in a small degree, I made the following observations of the Sun's eclipse.

* Phil. Trans. 1766, p. 77.

App. Time.

	h	'	"		
At 18	33	45		Beginning of the eclipse.	
	36	4		The double spot (<i>a</i>) immerses.	
	36	15		covered.	
	46	circ.		A large irregularity on the Moon's edge appeared on the Sun's disk.	
	54	44		The Moon touches the haziness surrounding the largest spot (<i>b</i>).	
	55	15		Nucleus of the spot (<i>b</i>) immerses.	
	56	11		D° covered.	
18	57	7		Haziness of (<i>b</i>) covered.	
19	5	12		The Moon touches the haziness surrounding the spot (<i>c</i>).	
	6	14		The nucleus immerses.	
	6	38		covered.	
	8	14		The spot (<i>d</i>) covered.	
	10	14		The spot (<i>f</i>) covered.	
	10	28		The spot (<i>g</i>) covered.	
	16	8		The spot (<i>b</i>) immerses.	
19	16	32		The spot (<i>b</i>) covered.	' "
19	17	51		Lucid parts measured	15 15,7
	18	37			15 12,7
	19	11			15 9,5
	19	58			15 1,5
	20	52			14 53,0
	21	28			14 49,7
	22	12			14 44,5
	22	48			14 41,3
	23	38			14 40,4
	24	28			14 39,5
					App.

App. Time.				
	h	'	''	
At	25	8		14 44,6
	25	44		14 48,3
	26	31		14 49,8
	27	19		14 50,5
	28	19		14 54,5
	28	58		14 59,1
	29	38		15 1,9
	30	15		15 4,7
19	52	37	The spot (c) totally uncovered.	
	55	17	(b) begins to emerge.	
	55	39	(b) totally uncovered.	
20	15	30	(i) totally uncovered.	
20	18	36	Eclipse nearly ended.	
20	18	42 $\frac{1}{2}$	Eclipse ends.	

The spots are marked with letters in the order in which they were covered by the Moon.

Many irregularities were observable upon the Moon's limb; though none of them were so pointed as some which I observed in the eclipse of the Sun, on August 16, 1765.

Towards the end of the eclipse, the sky began to be hazy; which haziness increased, and was very considerable at ten o'clock in the morning.

On the top of New College Tower, the Reverend Mr. Lucas, Fellow of New College, with an excellent acromatic telescope of 6 feet, magnifying 60 times, was certain that the external contact of Ve-

nus with the Sun was passed at $7^h 6' 12''$ apparent time, having perceived a small impression upon the Sun's edge several seconds sooner; and the Reverend Mr. Clare, Fellow of St. John's College, with the same instrument, judged the thread of light to be completed at $7^h 24' 28''$, having observed the limbs to be in contact several seconds sooner.

The next morning Mr. Lucas observed the beginning of the Sun's eclipse at $18^h 33' 47''$, and the end at $20^h 18' 37''$.

Mr. Sykes, of Brazen Nose College, with an acromatic refractor of $3\frac{1}{2}$ feet, made by Mr. Dollond, first saw Venus upon the Sun at $7^h 6' 0''$, and observed the thread of light to be completed at $7^h 24' 22''$.

Mr. Shuckburgh, of Balliol College, observed there the external contact of Venus with the Sun at $7^h 6' 8''$ apparent time, and the internal contact at $7^h 24' 25''$; though at $7^h 23' 16''$, he judged that the center of the Planet was removed more than its own semi-diameter from the Sun's limb, or that the true internal contact was then actually past. He is of opinion that the observation of the completion of the thread of light could not be made nearer than to $8''$ or $10''$, on account of the undulation of the limbs: and he farther adds, that when Venus was wholly entered upon the Sun, he could no longer perceive the penumbra that attended the Planet before the apparent contact; but that in the room of it there appeared a small circle of light, somewhat more luminous than the surrounding parts of the Sun. Mr. Shuckburgh also observed the beginning of the eclipse at $18^h 33' 51''$,
5
and

and the end at $20^h 18' 38''$, with the appulse of the Moon to several of the spots.

In an unfurnished room of the Hospital, that commanded the north-west part of the horizon, Mr. Nikitin of St. Mary Hall, and inspector of the Russian gentlemen sent here for their education by the Emperess of Russia, and Mr. Williamson, of St. Alban Hall, both well versed in the Mathematics, made the following observations of the transit, with a reflector of 10 inches, and a refractor of 8 feet :

	1st ext. cont.			Ingress.		
	h	'	''	h	'	''
Mr. Nikitin	7	6	44	7	24	$15\frac{1}{2}$
Mr. Williamson	7	6	29	7	24	$10\frac{1}{2}$

The transit and the eclipse were also observed here by the Reverend Mr. Horsley, F. R. S. and Mr. Cyril Jackson, A. B. and student of Christ Church. But as Mr. Horsley proposes to lay the observations before the Society, I have only to add, that I believe them to have been made with all the accuracy and care that the circumstances of the time and place would permit; and that those gentlemen are not less distinguished by their zeal for astronomical and mathematical inquiry, than for their extensive knowledge and erudition.

The latitude of Oxford is $51^{\circ} 45' 15''$, as determined by myself, from several observations of the Pole Star, both above and below the Pole, with an excellent mural quadrant, of 32 inches, made by Mr. Bird; the focal length of the telescope being
34 inches.

34 inches. I am the rather induced at present to mention this, as the latitude of Oxford, given by Mr. De la Lande in the *Connoissance des Temps*, and attributed to me, was determined by the late Professor Bliss, from observations made with a smaller and less perfect instrument. The longitude of Oxford is $5^{\circ} 3''$ or $5^{\circ} 4''$ to the west of Greenwich, the former quantity being deduced from a comparison of the Sun's eclipse, observed by myself, with Mr. Short's observation, an allowance being made in the computation for the figure of the earth, in the effect of the Moon's parallax.

Oxford, June 14,
1769.

Thomas Hornsby.

XXIV. *Venus observed upon the Sun at Oxford, June 3, 1769: By Samuel Horsley, L.L.B. Rector of St. Mary, Newington, in Surrey, F.R.S.*

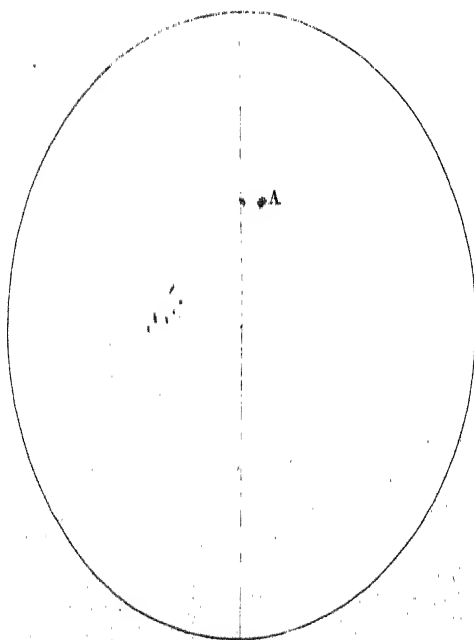
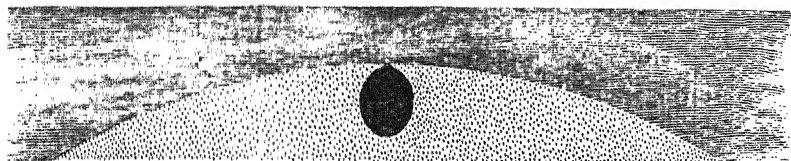
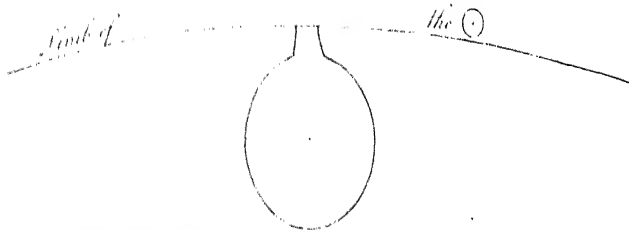
Read June 15, 1769. **M**Y regulator was moved to the place of observation on Wednesday evening, and set a going on Thursday; and, between that time and nine o'clock on Sunday morning, many comparisons were made of it with Mr. Hornsby's observatory clock, by which its rate of going and difference from Mr. Hornsby's clock, at the time of observation, were pretty well determined. At 10' before seven, by my regulator, I began to observe and to count the seconds, and about 3' and 3 or 4'' after seven, I descried a very small black notch on that part of the Sun's limb where I expected the Planet; but it was then so small, that I was in doubt whether it was any thing more than an appearance occasioned by the horizontal vapours, which were more copious than I could have wished, and made the Sun's edge, as usual, appear ragged in many parts. But by 5' after seven, this notch was grown so large, that no doubt remained with me that it was the Planet.

This was my observation of the external contact, which I write, however, chiefly by recollection; for, having had no experience of this observation before,
not

not having observed the transit of 1761, I had conceived a prejudice that it would not be possible to observe the external contact with any accuracy, and therefore I neglected to make any other minute of what I saw of it, but that I was certain that the Planet was upon the Sun by 5' after seven, by my regulator. Mr. Cyril Jackson, a student of Christ Church, who observed in the same room with me, told me, when all was over, that he thought he had notice of the Planet's approach, by a more vehement undulation in that part of the Sun's limb where the Planet entered than in any other, which he perceived a very short time before he saw the Planet. I confess that I was not sensible of this circumstance. I observed with an 18 inch reflector; Mr. Jackson used a refractor of Mr. Dollond's of nine feet. The wind was high, and very troublesome to both of us, by the motion it gave to our instruments.

When the Planet had been so long upon the Sun's limb, and so large a part of its circle was plainly entered, that I thought the internal contact was near at hand, I was much astonished to find the shape of the black spot suddenly altered from a large segment of a circle, to what I have attempted to express very rudely by a sketch, see TAB. VII. Fig. 1. where the lower part, which still seemed the segment of a circle, is connected with the Sun's limb, by a kind of ligament of darkness terminated on each side by right lines. The ligament detached itself from the Sun's limb; and the light, as I thought, was visible, all round the Planet, at $7^h 21' 52''$, by my regulator, and not earlier to my eye. And this I set down as the internal contact. The moment that I perceived the

the



the ligament detached from the Sun's limb, I turned my eye to the clock, to catch the minute, and to be satisfied that I was right in my counting of the seconds. And when I returned my eye to my telescope, which was before or not later than the 55th second, I found that the thread of light between the limb of Venus and the limb of the Sun had sensible breadth, and the shape of the Planet was perfectly circular. Mr. Jackson reckoned the internal contact at $7^h 21' 51''$, by our regulator. He judged of it as I did, by the detachment of the ligament, which he saw, as well as I, from the Sun's limb. My regulator, when it was first set a-going, seemed to gain on Mr. Hornsby's observatory clock for some hours (the pendulum, perhaps, not being come to its natural swing). But on Friday evening, about a quarter after eight, it was too slow for Mr. Hornsby's clock $13''$. On Saturday, half an hour after noon, it was $18''.9$ too slow. And Saturday evening, at $9^h 30'$, it was $27''$ too slow; and on Sunday morning, about nine o'clock, it was $32''$ or $33''$ too slow. So that at the time of the internal contact it was $25'' \frac{1}{2}$ or $26''$ too slow for Mr. Hornsby's observatory clock.

I was much surprized, upon comparing notes with Mr. Hornsby, to find that he had judged the internal contact $14'' \frac{1}{2}$ or $15''$ earlier than I did.

The foregoing narrative of what I saw, I have drawn up June 8th, having not conversed with any other observers, except Mr. Hornsby and Mr. Jackson, and Mr. Maskelyne, whom I met in the street this day, and talked with him very cursorily. And that my account may be purely of what I saw, as it

struck me at the time, before my own ideas were blended with those of other people, and altered (as may sometimes be the case by communication) from what they originally were in my own mind, I shall present this hasty memoir as I have drawn it up, without any correction or alteration.

June 8,
1769.

Samuel Horsley.

N. B. The figure that I have given does not (I believe) express accurately the proportion of the ligament to the circular segment of Venus's disk. I think that the right lines, which terminated the ligament, did not go off from the limb of Venus in angles quite so sharp as my figure exhibits. Nor do I think their convergence was so great as I have drawn it.

June 10, 1769.

June 13, 1769. Since I wrote the above, I have received from Mr. Hornsby a minute of the difference of his clock from mean time, at the time of observation, which I forgot to bring away with me from Oxford, and therefore could only state my observation before in the times of my own regulator and Mr. Hornsby's clock. I now subjoin my observations reduced to mean time at Oxford, reckoning Mr. Hornsby's clock too fast for mean time by $5'' \frac{1}{2}$ at the hour of observation.

External

		h	'	"
External contact	—	7	3	23 $\frac{8}{10}$
Detachment of the ligament		7	22	12 $\frac{1}{2}$

I have likewise obtained from my brother, Mr. John Horsley, a minute of his observation made at Greenwich with an excellent refractor of Mr. Dollond's, which magnified, however, only 50 times. My brother assures me, that he did *not* see the ligament which I have described, though it was seen by Mr. Maskelyne and by others, at Greenwich. He has set down, however, two different dates of the total ingress. One, which he calls close contact without any light, appearing between the limbs of Venus and the Sun, at $7^h 28' 15''$, apparent time at Greenwich. Another, which he marks thus, "a thread of light, fine as you can imagine, appearing between," at $7^h 29' 28''$. Here is an interval of $73''$ between the close contact and the appearance of light. The time of the appearance of the light being reduced to mean time, and to the meridian of Oxford (reckoning the meridian of Oxford $5' 4''$ west of Greenwich, as it is stated in Mr. Maskelyne's Tables), was $7^h 22' 9''$, which is only $3''$ earlier than my observation of the detachment of the ligament. Now from hence I conclude, that the magnifying power of the telescope, which my brother used, was too small to *show him the shape* of the ligament, yet the ligament had its effect with respect to obstructing the Sun's light, which he perceived about the same time as others, who used glasses of greater force; which seems to be a strong confirmation of the *reality* of what we saw: or that there actually was a part of the Sun's disk, which

remained obscure (from what cause I do not at present enquire) for several seconds after the limbs of the Planet and the Sun were separated. I think this worthy of remark, because I hear that the appearance of the ligament, which I have described, has been imputed by some to an inaccurate adjustment of the glasses to the observer's eye.

S. Horsley.

In the foregoing Paper, I have given several comparisons of my clock with Mr. Hornsby's. Its difference from Mr. Hornsby's, by a mean of all the comparisons, will be found $25''\frac{1}{2}$, at the time of observation. But I rely chiefly on the comparisons of Saturday night and Sunday morning, which make the difference $26''$.

XXV. *Observations of the last Transit of Venus, and of the Eclipse of the Sun the next Day; made at the House of Joshua Kirby, Esquire, at Kew*. By John Bevis, M. D. F. R. S.*

Read June 15, 1769. **I**N the morning of June 2, 1769, I fixed my equal altitude instrument, and carefully rectified it; and, applying the proper correction to the fore and afternoon's corresponding altitudes of the Sun, I found that Mr. Kirby's clock, whose rate of going was well regulated to mean solar time, at noon was 2' 5'' before the mean time; whence I deduced the apparent times of my observations. June 3, in the evening, I was alone in a room where I had a very commodious view of the Sun. My telescope was a very good reflector, of about three feet and a half focal length, with an aperture of near six inches, and a magnifying power of 120 times; it was steadily supported, and governed by rack-work, and I had a stop-watch in my hand. Mr. Kirby at the clock.

* Mr. Kirby's house is exactly $4''\frac{3}{4}$ of time east of his Majesty's domestic observatory, and $1' 14''$ west of the Royal observatory at Greenwich.

App. time.

a h ' "

- June 3, 7 9 59 I perceived a sudden boiling or tremor at the very summit of the Sun's limb, very different from what we usually call an undulation of his limb: 8
- 10 7 or 9'' after which, I called out *now!* upon discerning, at the same place, a very small indentation of Venus. I think I may put the external contact about 3'' sooner.
- 28 8 The Planet seemed quite entered upon the disk, her upper limb being tangential to that of the Sun: but, instead of a thread of light, which I expected immediately to appear between them, I perceived Venus to be still conjoined to the Sun's limb by a slender kind of tail, nothing near so black as her disk, and shaped like the neck of a Florence flask.
- 28 17 The said tail vanished at once, and, for a few seconds after, the limb of Venus, to which it had been joined, appeared more prominent than her lower limb, somewhat like the

the lesser end of an egg, but soon resumed its rotundity.

In a few minutes more the whole circumference of Venus became very ill defined, and beset with asperities, which I have represented, as well as I could, in my Figure (TAB. VII. fig. 2.). These were amazingly agitated by a sort of curling, quick motion not easily to be described. A gentleman of my acquaintance fancied Venus, in this circumstance, to resemble a black wafer on the head of a beaten drum. In the transit of Venus, in 1761, which I observed at Savile-house, I saw not the least of such appearance at the exit. The Planet was then perfectly circular and well defined.

The sky, though for the most part of the day clouded over, was all this while very fine.

a	h	'	
18	36	16	The Sun's eclipse began, perhaps, 2 or 3'' sooner.
	59	24	The spot A bisected. (See fig. 3.)
20	22	33	The eclipse ended, very exact.

J. Bevis.

XXVI. *A Letter to the Astronomer Royal, from John Canton, M. A. F. R. S. containing his Observations of the Transit of Venus, June 3, 1769, and of the Eclipse of the Sun the next Morning.*

London, June 9, 1769.

S I R,

Read June 15, 1769. **I** TAKE the liberty of sending you, inclosed, my observations of the transit of Venus, and of the Sun's eclipse; which, if you think proper, you may lay before the Royal Society. Those of Venus were made under the disadvantageous circumstances of being at the top of a house, and seeing through smoke; however, I hope they will not differ very widely from yours. I was just prepared to find the difference of declination between the Sun and Venus by your method, which appears to me to be a very good one, when the Sun was covered by a cloud, and I saw it no more. The magnifying power of the telescope I made use of was 95.

About half a minute before the total ingress, when the bright cusps of the Sun were at some distance from each other, there appeared a faint light between them, a little lower than the cusps, or nearer

to the center of the Planet: this I observed to increase till the time of the internal contact; which fully convinced me that there is an atmosphere about Venus.

I had the good fortune to take several correspondent altitudes of the Sun on the day of the transit, and also on the day before.

The longitude of Spital Square, west of the Royal observatory, I formerly found by Rocque's survey, to be $16''\frac{3}{4}$ of time; and lately, by observing with you the explosions of rockets, it was found to be $17''\frac{1}{10}$. I therefore add $17''$ to my time, to bring it to yours.

I am, Sir,

Your humble servant,

John Canton.

Spital Square, June 3, 1769.

OBSERVATIONS OF THE TRANSIT OF VENUS.

	h	'	"	
1st external contact at	7	8	$28\frac{1}{2}$	} mean time.
1st internal contact at	7	26	$59\frac{1}{2}$	

Duration of the ingress	18	31
-------------------------	----	----

	h	'	"	
Equation of time	2	$15\frac{3}{4}$		add

	h	'	"	
1st external contact at	7	10	$44\frac{1}{4}$	} apparent time.
1st internal contact at	7	29	$15\frac{1}{4}$	

The diameter of the Sun, from 3 observations, was $31\ 35\frac{1}{2}$
of Venus, from 4 observations, 59

At $7^h\ 38'\ 31''$, apparent time, the right ascension of γ was greater than that of the \odot by $8'\ 7''$.

[194]

OF THE SUN'S ECLIPSE.

	h	'	"	
The beginning at	18	36	40	} mean time.
End at	20	21	7	

Duration	1	44	27
----------	---	----	----

Equation of time	2	10	$\frac{3}{4}$
------------------	---	----	---------------

	h	'	"	
Beginning at	18	38	$50\frac{3}{4}$	} apparent time.
End at	20	23	$17\frac{3}{4}$	

Digits eclipsed	6	$14\frac{1}{2}$
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At	h	'	"	apparent time.	dig.	'	eclipsed.
19	20	45	$\frac{3}{4}$		6	$1\frac{1}{2}$	
19	52	45	$\frac{3}{4}$		4	$37\frac{1}{4}$	
20	—	44	$\frac{3}{4}$		3	$33\frac{1}{2}$	

XXVII. *An Account of several sepulchral Inscriptions and Figures in Bas-relief, discovered, in 1755, at Bonn in Lower Germany. In a Letter to James West, Esquire, Pr. R. S. from John Strange, Esquire, F. R. S.*

S I R,

Read Nov. 9, 1769. **Y**OUR distinguished taste for antiquities, and the zeal you have ever shewn in the preservation of them, encourage me to address to you the following account of some curious remains of Roman antiquity, found a few years since at Bonn in Lower Germany.

In a journey to Italy, in the summer of 1757, passing through Germany and the Tyrol, I had opportunities of collecting several ancient Roman inscriptions, which, upon examination afterwards, I found had never been published. Upon my arrival in Tuscany, I therefore communicated them to such, among my literary friends, as delighted in these studies, and my German inscriptions were soon after published by Dr. Lami, professor of theology at Florence (1).

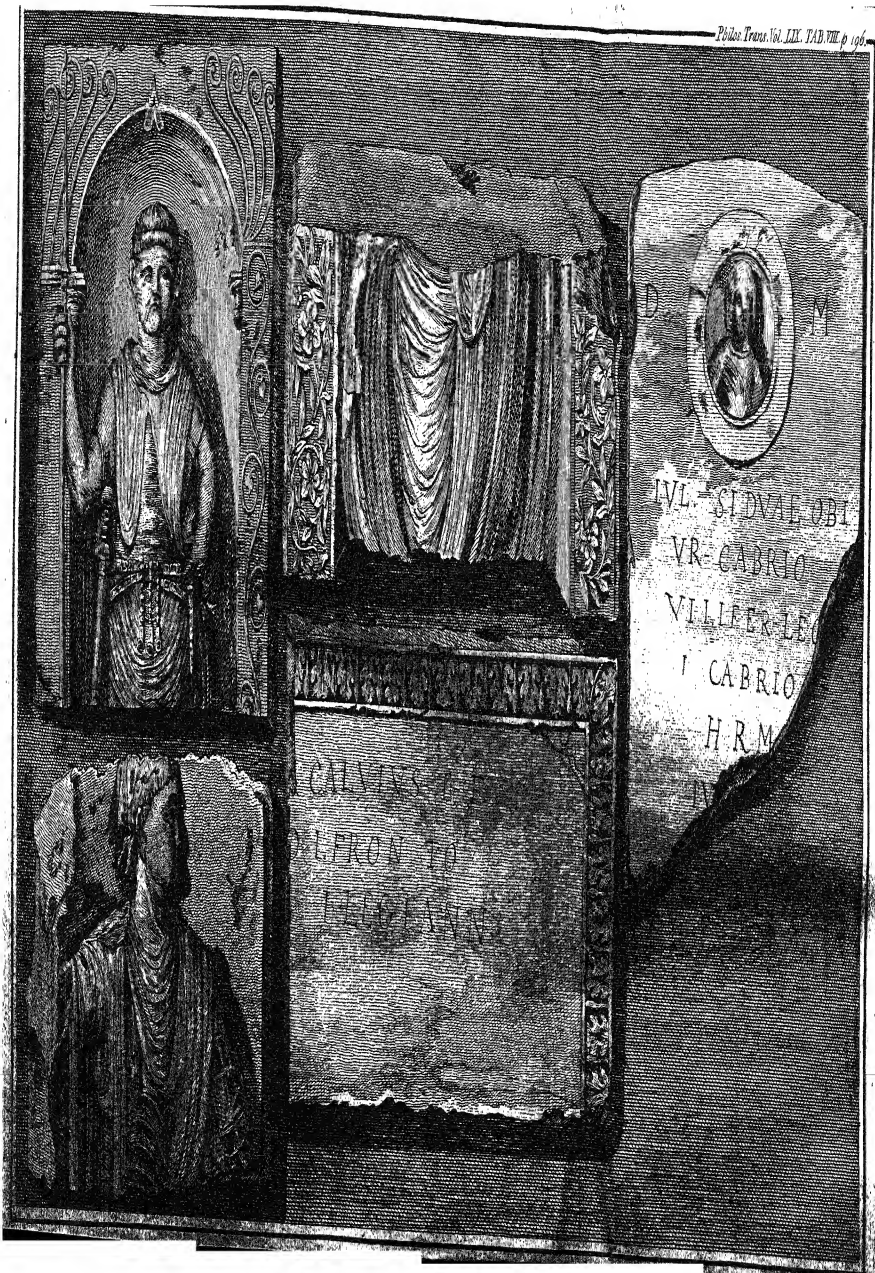
(1) *Novelle Letterarie di Firenze*, t. xxiv, p. 527:

The remainder were inserted by Abbé Donati, of Lucca, in a new collection of ancient Roman inscriptions which he has lately published by way of supplement to Muratori's Thesaurus. But as several of the inscriptions, which I had collected in Germany, were copied from bas-reliefs, which appeared to me to be curious, and very little known, I thought proper to have drawings of them made, and take this opportunity of communicating them to you; desiring at the same time, that you would be pleased to present them, in my name, to the learned Society over which you preside, should you think they merit their attention and acceptance. Permit me at present to give you the following short account of the originals.

About May, 1755, in digging some foundations in a garden belonging to his serene Highness the Elector of Cologne, at Bonn in Germany, several ancient Roman sepulchral stones were found. Eight of these, being thought curious on account of the bas-reliefs and inscriptions carved upon them, were soon after fixed up against the wall of an inner open court of the electoral palace at Bonn, where they still remain.

The drawings(2), which I have now the honour to send you, are faithful copies of these antiquities, which being in the highest preservation, and the inscriptions upon them containing nothing more than the usual form, it would be impertinent to enter into any particular description of them. I cannot however omit remarking the singular barbarity of the Roman soldiers names in these inscriptions. It will be further

(2) See Tab. VIII. and IX.

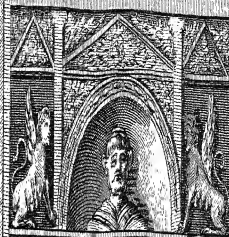




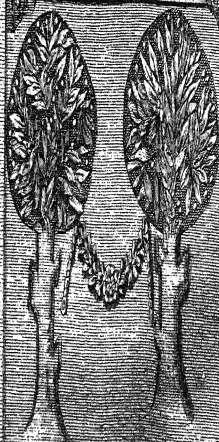
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Received June 19, 1769.

XXVIII. *An Account of the Lymphatic System in Amphibious Animals. By Mr. William Hewson, Lecturer in Anatomy: In a Letter to William Hunter, M. D. F. R. S. and by him communicated to the Society.*

S I R,

Read Nov. 9,
1769.

A GR E E A B L E to the promise which I made, in a postscript to the paper on the lymphatic system in birds, which the Royal Society did me the honour of publishing in their last volume, I now take the liberty of begging of you to present to the learned Society an account of the same system in a turtle. I should likewise have added a figure of that animal, had not these vessels agreed in so many particulars with those in birds, that I flattered myself the description would be intelligible without it.

This system in a turtle, like that in birds, consists of the lacteals and lymphatics, and their common trunks, or the thoracic ducts. It agrees likewise with that in birds, in not having any visible lymphatic glands either on the mesentery or near the thoracic ducts; but

differs from that in birds, in not having any glands upon the larger lymphatics of the neck; at least I am inclined to believe so, from not having seen any in the dissection of one animal of this species in which I looked for them. It likewise differs from that in birds in another circumstance, to be taken notice of hereafter. Whether it agrees with the same system in birds, in the transparency and want of colour in the chyle, I cannot take upon me to determine, as I did not see any of that fluid in this subject (a).

The following description I took from the animal, after I had injected the larger branches of this system with a coloured wax, and the smaller with quicksilver. To avoid my being misunderstood, when I speak of the situation of the different parts, I shall mention, once for all, that the description was taken from the subject as it lay upon its back; those parts being called highest which were nearest the head, those lowest which were nearest the tail, those posterior which were nearest the back, and those anterior nearest the belly (b).

The lacteals accompany the blood-vessels upon the mesentery, running by their sides, and communicate frequently across those vessels. Near the root of the mesentery they anastomose, so as to form a net-work, from which several large branches go into some considerable lymphatics lying on the left side of the spine.

(a) In a crocodile, which I lately saw by the favour of Mr. John Hunter, the chyle was white.

(b) The animal, from which I took this description, was large, measuring from the lower to the upper part of the shell two feet seven inches, and two feet two inches from side to side.

These

These last can be traced downwards almost to the *anus*, and belong to the parts situated below the mesentery, and particularly to the kidneys. At the root of the mesentery, on the left side of the spine, the lymphatics of the spleen join the lacteals, and immediately above this union a sort of *plexus*, or network, is formed, which lies upon the right *aorta* (for there are two *aortæ* in this animal). From this *plexus* a large branch arises, which passes behind the right *aorta* to the left side, and gets before the left *aorta*, where it assists in forming a very large *receptaculum*, which lies upon that artery. From this *receptaculum* arise the thoracic ducts. From its right side goes one trunk, which is joined by that large branch which came from the *plexus* to the left side of the right *aorta*, and then passes over the spine. This trunk is the thoracic duct of the right side; for, having got to the right side of the spine, it runs upwards on the inside of the right *aorta*, towards the right subclavian vein. And when it has advanced a little above the lungs, or within three or four inches of the subclavian vein, it divides into branches, which, near the same place, are joined by a large branch that comes up on the outside of the *aorta*. From this part upwards those vessels divide and sub-divide, and are afterwards joined by the lymphatics of the neck, which likewise divide into branches before they join those from below; so that between the thoracic duct and the lymphatics of the same side of the neck a very intricate net-work is formed. From this net-work a branch goes into the angle made by the jugular vein and the lower part or trunk of the subclavian: this branch, therefore, lies on the inside of the
jugular,

jugular, whilst another gets to the outside of that vein, and seems to open into it a little above the angle between that vein and the subclavian. I say seems to open, for the injection has not succeeded at this part so as to enable me to determine whether the last-mentioned branch really did enter or not. Into the above-mentioned *receptaculum*, the lymphatics of the stomach and *duodenum* likewise enter. Those of the *duodenum* run by the side of the *pancreas*, and probably receive its lymphatics, and a part of those of the liver. The lymphatics of the stomach and *duodenum* have very numerous *anastomoses*, and form a beautiful net-work on the artery which they accompany. From this *receptaculum* likewise, besides the trunk already mentioned, which goes to the right side, arise two other trunks pretty equal in size; one of which runs upon the left side, and the other upon the right side of the left *aorta*, till they come within two or three inches of the left subclavian vein; where they join behind the *aorta*, and form a number of branches, which are afterwards joined by the lymphatics of the left side of the neck: so that here a net-work, or *plexus*, is formed, as upon the right side. From this *plexus* a branch issues, which opens into the angle between the jugular and the lower part or trunk of the subclavian vein. In these net-works, formed by the lymphatics near their terminations in the veins, this system in the turtle likewise differs remarkably from that in birds.

So much for the general description of the lymphatic system in this animal, I shall next add what I have observed as to the more minute distribution of its lacteals. In the first place, it may be observed, that

what knowledge we have of the minute distribution of those vessels in quadrupeds has been acquired from examining them when filled with their natural fluid, the chyle; for the valves with which those vessels abound prevent our injecting their smaller branches; as we do those of the arteries and veins of the intestines. But in this animal, I have been so fortunate as to force the valves, and to inject the lacteals from their trunks to their branches, so as to fill them all around with quicksilver, in several parts of the intestine. In these experiments I observed, that the quicksilver was often stopped by the valves, where the lacteals run upon the mesentery, or where they are just leaving the intestine; but when those valves were forced, and the quicksilver had once got upon the surface of the gut, it generally ran forward without seeming to meet with any obstacle. The lacteals anastomose upon the intestines, so that the quicksilver, which has got upon them by one vessel, in general, returns by another, at some distance. The larger lacteals, which run upon the intestines, accompany the blood-vessels; but the smaller lacteals neither accompany those vessels, nor pass in the same direction, but run longitudinally upon the gut, and dip down through the muscular coat into the cellular or nervous, as it has been called, which in this animal is very thin in comparison to what it is in the human subject. So far I have traced those vessels to my satisfaction; but what becomes of them after they have got to the cellular coat is not so easy to determine: in five or six different experiments which I have made, the mercury passed from the lacteals into the cells between the muscular coat and the internal,

and

and spread from cell to cell, very uniformly, over a great part of the intestine, although but little force had been used, and although there was nothing like extravasation in any other part of the intestine. Upon inverting the intestine after thus filling its lacteals, the mercury, on being pressed, was, in many parts, driven into small vessels upon the internal coat, or villous, as it is called. From this it would seem, that this cellular net-work was a part of the lymphatic system in this animal. It might indeed be supposed to be mere extravasation, but that it is rather a part of the lymphatic system appears probable from the following considerations. First, from the regularity in the size of the cells. Secondly, from the little force used in the experiment, and from there being nothing like this appearance in the cellular membrane between the peritoneal and muscular coats where extravasations were as likely to happen. Thirdly, from my having been able, after inverting the intestine, to press the quicksilver from the cells into the very small vessels upon the internal coat; but I must confess these facts would not be sufficient to determine whether these cells were, or were not a part of the system, did not the analogy of the same part in fish clearly prove it. For in the cod, instead of the cellular net-work, as in the turtle, there is a network of vessels (of which a description shall be given hereafter) so that I have now no doubt but that those cells are parts of the lymphatic system, and that the small absorbent vessels of the internal coat pour their fluid into this net-work, from which it is conveyed by the larger lacteals.

Received June 19, 1769.

XXIX. *An Account of the Lymphatic System in Fish. By the same.*

Read Nov. 16, 1769. **I**N the foregoing paper on the lymphatic system in a turtle, I have made no mention of the manner of discovering, and demonstrating those vessels; the reason was, there is no difficulty in either the one or the other; for, in that animal the mesentery being very thin and transparent, and the lacteals pretty large, they are more readily discovered than in any other animal; thence it happened that I saw those vessels in a turtle long before I discovered them in birds and fish, and that too by chance, and when I was not intent upon this inquiry. And since I drew up the preceding account, I have found that Mr. John Hunter, in a description of the structure of a crocodile, had mentioned, by the way, that it had lacteals. Professor Monro, of Edinburgh, as I have been informed, likewise saw the lacteals in a turtle about four years ago. As it is natural for men, engaged in the same inquiries, to be desirous of ascertaining their priority in the discoveries, even when there is little merit in the making them, I therefore take this opportunity of doing justice to those gentlemen, and at the same time

time of mentioning that I first saw those vessels, as near as I can recollect, in September or October 1763, which, as far as I know, was before they had been seen by either of them.

But although it was an easy matter to see those vessels in the turtle, yet it was far from being so in birds and fish; as the gentlemen of the Society will readily believe, from their having been so often sought for in vain by so many eminent anatomists, particularly of this age. I may add, that the discovery in birds did not give me so much trouble as that in fish, though now, since I have seen them in the latter, I can more readily find them there than in birds or quadrupeds. After seeing them in birds, and in one of the *amphibia*, I was very desirous of determining whether fish were, or were not provided with those vessels. This I endeavoured to do in the same way that I had found them in birds, that is, by tying up the mesenteries of live-fish; and for this purpose I went frequently to the markets, and examined several small ones. I likewise dissected some larger, when dead, but in vain. I next went to Brighthelmstone, where I found kingston, or monk-fish, a species of skate. These being very large, and having a lean mesentery, seemed well fitted to my purpose. I opened two of them alive, tied up their mesenteric vessels, and put them again into the salt water; and though one of them lived an hour, I could not observe any lacteals either upon its intestine or its mesentery. After this, I repeatedly examined the intestines and mesenteries of common skate and cod, and at last was so fortunate as to discover the lacteals, and get a pipe into one of those vessels on the mesenteries of each of these.

these fish; and, by injecting by this pipe, I found where the larger vessels lay; after which there was but little difficulty in tracing the whole system. I have now seen those vessels in a variety of fish, and shall give a description of them from a haddock. I shall proceed exactly in the order which I have found most convenient for tracing out the whole system for demonstration, beginning with one of its branches, which, as lying nearest the surface, must, of course, be divided before the other parts can be exposed to view. The account being taken from the fish as it lay on its back, those parts are called superior which are nearest the head; those inferior, which are towards the tail, those posterior which are towards the back, and those anterior which are towards the belly.

On the belly of the fish, exactly in the middle line, is a lymphatic, which runs from the *anus* upwards; this lymphatic belongs not only to the *parietes* of the belly, but to the fin below the *anus*. It runs up towards the head, passes between the two jugular fins, and, having got above them, it receives their lymphatics. It then goes under the *symphysis* of the two bones which form the *thorax*, where it opens into a net-work of very large lymphatics, which lies close to the *pericardium*, and almost intirely surrounds the heart. This net-work, besides that part of it behind the heart, has a large lymphatic on each side, which runs upon the bone of the *thorax* backwards, and when it has got as far as the middle of that bone, it sends off a large branch from its inside to join the thoracic duct. After detaching this branch, it is joined by the lymphatics of the pectoral fins, and

soon after, by a lymphatic which runs upon the side of the fish. This last-mentioned vessel consists of a trunk running on the side just opposite to the ribs, and from this trunk proceed branches on each side immediately under the skin; so that it has a beautiful penniform appearance. Besides these branches, there is another set deeper seated, which accompany the ribs. After the large lymphatic has been joined by the above-mentioned vessels, it receives the lymphatics from the posterior extremities of the gills, and having now got as far back as the orbit, it next receives lymphatic vessels from that cavity; but these vessels do not belong merely to the orbit; for one of them comes from the nose, and another from the upper part of the mouth. A little below the orbit, another net-work appears, consisting, in part, of the vessels above described, and of the thoracic duct. This net-work is very complex; some of its vessels lie on each side of the muscles belonging to the gills, and from its internal part a vessel goes into the jugular vein, by which vessel the whole system is terminated. The large lymphatic above mentioned, which lies upon the bone of the *thorax*, has likewise a process running towards the upper part of the kidney, and receives some of the lymphatics of that organ.

The lacteals run on each side of the mesenteric arteries, anastomosing frequently across those vessels. The *receptaculum*, into which they enter, is very large in proportion to them, and consists, at its lower part, of two branches, of which one lies between the *duodenum* and stomach, and runs a little way upon the *pancreas*, receiving the lymphatics of the liver, *pancreas*, those of the lower part of the stomach, and the

the lacteals from the greatest part of the small intestines. The other branch of the *receptaculum* receives the lymphatics from the *rectum*, and the lacteals from the greatest part of the small intestines. The *receptaculum*, formed by these two branches, lies on the right side of the upper part of the stomach, (or the lower part of the *œsophagus*) and is joined by some lymphatics from that part; and also by some small vessels from the sound, and from the gall bladder, which, in this fish, adheres to the *receptaculum*. The thoracic duct takes its rise from the *receptaculum*, and lies on the right side of the *œsophagus*, receiving lymphatics from that part; and running up a little way (viz. about half an inch in this fish) it divides into two branches or ducts, one of which passes under the *œsophagus* to the left side, and the other goes straight up, on the right side, runs past the upper part of the kidney, from which it receives some small branches, and soon after it is joined by a branch from the large lymphatic that lies above the bone of the *thorax*, as formerly mentioned. It likewise, near this part, sends a branch to join the duct of the opposite side, and then, a little higher, is joined by those large lymphatics which make a net-work behind the heart, as formerly described. These last mentioned vessels receive the lymphatics from the anterior, or superior part of the gills, and from the *fauces*. The thoracic duct, after being joined by these vessels, communicates with that net-work near the orbit; where its lymph is mixed with that of the lymphatics from the posterior part of the gills, from the superior fins, belly, &c. and then from this net-work a vessel goes into the jugular vein, just below the orbit. This last vessel,

vessel, which I called the termination of the whole system, is very small, in proportion to the net-work from which it rises; and indeed the lymphatics, at this part, are so large as to exceed by far the size of the sanguiferous vessels.

The thoracic duct of the left side, having passed under the *œsophagus* from the right, runs on the inside of the *vena cava* of the left side, receives a branch from its fellow of the opposite side, and joins the large lymphatics which lie on the left of the *pericardium*, and a part of those which lie behind the heart, and afterwards makes, together with the lymphatics from the gills, upper fins, and side of the fish, a net-work, from which a vessel passes into the jugular vein of this side. In a word, the lymphatics of the left side agree exactly with those of the right, as above described.

Besides these vessels, there is yet another part of the system which is deeper seated, lying between the roots of the spinal processes of the back-bone: this part consists of a large trunk, that begins from the lower part of the fish near the tail, and, as it ascends, receives branches from the dorsal fins, and from the adjacent parts of the body. It goes up near to the head, and sends a branch to each thoracic duct, near the part where they come off from their common trunk.

This description, though taken from a haddock, agrees, I believe, pretty exactly with the distribution of those vessels in the cod, whiting, and perhaps all other fish of the same shape.

To this general description I shall add what I have observed of the more remarkable peculiarities of this system in fish.

In the first place, those vessels are remarkable in not having any lymphatic glands, that I can discover, in any part of their course. In this they agree with the turtle, but differ from birds, which have lymphatic glands on the vessels of their necks.

Secondly, these vessels in fish have no valves, so that it is an easy matter to fill them contrary to the course of the lymph. When I first observed this circumstance, I imagined that, by injecting minutely those vessels, I might discover their very beginnings, and that I might also be enabled to determine, whether such parts as the brain, eye, &c. whose lymphatics have not been yet seen in any animal, have, or have not, such vessels. At present I am not able to determine this matter, but I intend to prosecute the subject.

Thirdly, the lacteals in the cod (and I presume in most other fish) are remarkable for having a beautiful net-work of vessels between the muscular and vilous coat of the intestines (*c*). This net-work may be filled from the lacteals on the mesentery with the least force imaginable. If mercury be injected into this net-work at one part, it spreads over the intestine; the communications in the net-work being very numerous: if the intestine be inverted, and the mercury squeezed, it is easily driven into the small vessels of the *villi* of the internal coat. From these vessels the mercury can be squeezed into the cavity of the intestine; but not so easily as to make it clear whether they have, or have not, a valve at their beginning. In these circumstances there is a strong

(*c*) I have seen this net-work in the turbot, plaice, and cod.
analogy

analogy between fish and the turtle ; but in fish it is more evident that there can be no deception as to the net-work between the muscular and internal coats ; for in them it is made up of cylindrical vessels, and is not cellular, as in the turtle, and therefore not in the least like an extravasation : and in fish the vessels on the internal coat are larger than in turtles.

Fourthly, this system agrees with that of the turtle, in having a very large *receptaculum*, and in having the net-work of large vessels near its termination in the sanguiferous system ; and likewise in having the vessel, which goes from the net-work into the vein, small in proportion to the size of that net-work : so that the lymph must be lodged some time in those parts before it is poured into the mass of blood. In birds I also observed something like this, their lymphatic system being enlarged or varicose at different parts ; but these enlargements are small in proportion to those above mentioned in fish and turtle.

As to the manner of discovering those vessels in a fish, one might naturally suppose, that when we know where the *receptaculum*, or any of the larger parts of this system lie, it could not be difficult to find them ; but the coats of these vessels are so thin and transparent, that it is by no means easy. But the readiest way of finding the whole system is, to look for one of the vessels which lie close to the skin ; as, for instance, that which runs up exactly on the middle of the belly of the haddock, cod, and other fish of the same shape. This vessel is easily seen as it grows pretty large where it passes between the two jugular fins ; and if a pipe be introduced, the whole system may be filled by means thereof.

It is partly owing to the ease with which those vessels may be seen, after discovering where their larger branches lie, that I have not added a figure of this system in a fish. Indeed it would be almost impossible to express all its parts in one figure, from the numerous and intricate communications of those vessels near their termination in the common veins. But I have laid before the R. Society a haddock with its lymphatics and its blood-vessels filled with coloured injections, to be compared with the description. And those that are desirous of prosecuting this subject further will, I flatter myself, find it an easy matter to fill the whole system, by attending to what I have said above.

I shall beg leave to add one observation more ; and that is with respect to the distribution of the lacteals on the *villi* (as they are called) of the intestines. From a variety of experiments, which I have made, I am persuaded, that in animals, in general, each of the *villi* is composed of a net-work of lacteal vessels, as well as of a net-work of arteries and veins. The very ingenious Dr. Lieberkühn has endeavoured to shew, that in the human subject each lacteal forms an *ampullula* or oviform vesicle, which is filled with a spongy substance. But from having injected those vessels, with mercury, in fish, turtle, and birds, I can clearly demonstrate that each of the *villi* of these animals has a net-work of lacteals, and not an *ampullula* or oviform bag. And from comparing these observations with Dr. Lieberkühn's experiments, I am inclined to believe the structure is similar in the human subject. But as proposing my arguments at present would not be altogether agreeable to the design
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of this paper, I shall refer that subject to some future occasion. In the mean time, as I flatter myself that it may not be unacceptable to the learned Society, I have ready to lay before them a collection of preparations, which demonstrate not only these facts relating to the *villi*, but others that are mentioned in this and the preceding paper. Of these preparations here follows the list.

- N^o 1. A turtle, with its lymphatic and its sanguiferous systems filled.
2. A haddock, with its lymphatic and sanguiferous systems filled with injections of different colours.
3. The lymphatics on the stomach of a cod, filled with mercury; the arteries with a red, and the veins with a green injection.
4. The lymphatics on the stomach of a turbot.
5. The lacteals on the mesentery and small intestine of a cod. In this preparation the arteries are filled with a red, and the veins with a yellow injection, and the lacteals with mercury; and, the preparation being afterwards dried, the lacteals are seen to form a curious net-work of vessels between the muscular and the villous coat.
6. The same in a bottle.
7. The lacteals on the gut of a skate. In this preparation the lacteals are filled with mercury, the artery with a red, and the vein with a green wax.
8. The lacteals on the mesentery and intestines of a turbot. In this preparation the vascular net-work:

net-work between the muscular and villous coat is likewise very distinctly seen.

9. The mesentery and a piece of the intestine of a turtle ; where the lacteals are seen filled with mercury, not only on the mesentery, but on the whole external surface of the gut, forming a beautiful net-work.
10. The same. In this the arteries are filled with a red, the veins with a blue injection, and the lacteals with mercury, and the preparation being afterwards dried, the lacteals are seen not only on the mesentery and the external surface of the intestine, but the cellular net-work between the muscular and the villous coat is likewise distinctly seen.
11. The same, where the cellular net-work is filled all around the intestine.
12. Three preparations of the intestines of a turbot, in which the lacteals are seen to make a net-work on each of the *villi*.
13. Three^e more preparations from the same fish ; where the lacteals are filled with mercury, and the arteries and veins with a coloured injection ; each forming a net-work on the *villi*.
14. Three preparations of the gut of a turtle, where the lacteals on the *villi* of the internal coat are filled with mercury, and form a net-work.
15. A piece of the gut of a goose, in which the lacteals of the *villi* are filled with mercury ; the arteries with a red, and the veins with a yellow injection. In this preparation both
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the lacteals, arteries and veins are seen to make a net-work on many of the *villi*.

16. A piece of the stomach of a cod, in which the lymphatics are filled with mercury, and very minutely. They are seen to go through the external coats of the stomach, dividing into smaller and smaller branches, without any appearance of a net-work between the muscular and the villous coat; and as a considerable force was used in the injecting them, I am inclined to believe that the stomach in fish has not the net-work of vessels between the muscular and the villous coat, as the intestines have (*d*).

(*d*) These preparations were laid before the Society on the same evening this paper was read.

Received June 8, 1769.

XXX. *A Letter from Mr. Lane, Apothecary, in Aldersgate-street, to the Honourable Henry Cavendish, F. R. S. on the Solubility of Iron in simple Water, by the Intervention of fixed Air.*

Aldersgate-street, June 5, 1769.

S I R,

Read Nov. 23, 1769. **T**HE various impregnations of mineral waters have always been very difficult to explain: and whoever has read the divers, and often contradictory reasonings upon the subject, must clearly perceive, that there is still room for discoveries in this part of natural history.

You, Sir, by your accounts of fixed air, and of Rathbone-place water, related in the last volume of Philosophical Transactions, have obliged the public with many additional lights on this branch of knowledge; and, from your known accuracy, and diligent pursuits in most philosophical inquiries, the learned world has great reason to hope for many other new and useful improvements. To your judgment therefore, I submit the following experiments; which are intended to shew, that iron is soluble in simple water,
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by the intervention of fixed air; and thence, that it is very probable, many different chalybeate springs sustain their metallic charge by this means only.

The solution of iron in mineral waters, especially in such as, by exposure, readily lose the property of sustaining a purple colour with astringent vegetables, has usually been attributed to some subtle gas, or volatile acid. Chymistry, however, does not discover any acid solvent for iron, but what has greater affinity with alkalies; and by means of which, therefore, this metal will be precipitated. Hence if any water appears, with a predominant alkali, which has also the power of tinging with galls, and, on being exposed to the open air, lets fall the iron, and loses that property; may we not conclude the metal to have been suspended by some other medium?

This, for example, is plainly the case in German Spa water, which Dr. Brownrigg has proved to abound with fixed air. Your own very curious experiments, before cited, have clearly shewn, that calcarious earths may be suspended in water by this principle of fixed air. And these have led me to examine, whether iron might not be dissolved by the same natural means.

I would not, however, be supposed to deny, that iron is frequently found united with an acid. The fact is sufficiently evinced in the pyrites and vitriolic earths. Nor can I doubt, but that these substances do largely contribute to the primary impregnation of waters, they being so readily soluble in them. But as an alkali, or absorbent earth, is often found more than sufficient to saturate the acid in mineral waters; this would effectually disengage every

particle of iron dissolved by an acid, unless the metal was supported by some other menstruum.

My endeavours, therefore, to detect this solvent, by experiments, are what I now beg leave to lay before you, in the order I made them.

EXPERIMENT I.

A wide-mouthed bottle, containing half a pint of distilled water and sixty grains of steel-filings, was suspended forty-eight hours over some distillers melasses, in brisk fermentation; so as to receive the fixed air escaping from the fermenting liquor; the surface of which was ten inches below the mouth of the bottle. Immediately after its removal, the clear water was decanted from the filings and ochrous sediment.

This liquor had a brisk and ferruginous taste, with a flavour of the melasses. An infusion of galls, or green tea, soon changed part of it to a colour like ink. The remainder, being exposed to the open air, presently became turbid, threw up a party-coloured pellicle, and deposited a yellowish sediment.

The water now retained but very little power of tinging with galls; and in a few days lost this property entirely.

EXPERIMENT II.

Fourteen ounces of coarse sugar, dissolved in seven pints of water, were mixed with half a pint of yeast, in a bottle capable of holding more than twice the above quantity. One end of a bent tube was luted into this vessel, so that no air might escape but through
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the tube; the other end was loosely inserted two inches within the mouth of another large bottle, charged with four hundred grains of iron filings, and sixty ounces of distilled water. After remaining twelve hours in this situation, the sugar working briskly, an ounce phial was let down gently into the bottle, and filled. The water from the phial, with one drop of tincture of galls*, changed in a few minutes to a light rosy purple. The liquor being shaken, and another phial-full taken up soon after, one drop of the tincture gave a deeper colour than before. In an hour and half more, after being shaken again, the phial-full received a still deeper purple, from the like quantity of tincture. The bottle continuing as before near five hours longer; when the quantity of fixed air from the fermenting liquor was supposed sufficient to have saturated the water; the liquor appeared very turbid on being shaken; and, after standing another hour, under the tube, to settle, the whole was filtered.

Thirty ounces of the clear liquor was poured into two Florence flasks, and the remainder into phials, which were afterwards well corked. Two of these phials had their corks dipped into melted resin, so as to cover the mouths of the bottles. Two others were enclosed with a paste or lute.

Notwithstanding the above precautions to prevent the escape of air, the liquors in each soon grew turbid, and by the next morning deposited yellow sediments.

* This tincture was made by infusing half an ounce of powdered galls in eight ounces of proof spirit, for four days, without heat.

This water had a smart chalybeate taste, somewhat resembling Spa water; with a slight flavour of the fermenting liquor.

One drop of tincture of galls gave a rosy purple colour to a wine-pint of this water.

Syrup of violets turned it green*.

Soap leys, or even alkaline salts, either fixed or volatile in their natural states, soon changed this liquor green, and rendered it turbid, whence a yellow sediment ensued.

But neither of the alkaline salts, when previously saturated with fixed air, produced any perceptible alteration.

Nor did any visible change happen on the addition of acids.

The thirty ounces of water, in the flasks before mentioned, after being boiled twenty minutes, to expel the air, became very turbid, and let fall sediments. The clear liquor being decanted, the remainder was passed through a filter, and, after drying, the paper appeared to have gained two grains and a quarter.

This ochrous residuum could not be again dissolved in water, by means of fixed air; but was soluble in the vitriolic acid. The solution, diluted and filtered, received no colour from galls, until alkali was added to saturate the redundant acid; after which it struck a purple, as in common solutions of iron.

* Simple distilled water, saturated with fixed air, by any means I have tried, makes no change in syrup of violets: and, when mixed with soap, does not curdle.

The liquor, decanted after boiling, neither changed colour with galls, nor shewed any precipitation with lime-water.

EXPERIMENT III.

A common quart-bottle was half filled with distilled water, to which were added an hundred grains of steel-filings. To these was introduced, by means of the bent tube, as much fixed air, obtained from a solution of alkaline salt in the vitriolic acid, as was judged sufficient to fill the bottle. The whole being then shaken, with my hand over its mouth, the bottle stuck like a cupping-glass. About the same quantity of air being again added, the bottle, after shaking, had less adhesion than before. On repeating this experiment, a third time, with fresh air, the adhesion was scarcely perceptible. And after the fourth trial, a small portion of air was observed to issue from the bottle. The water now gave a deep colour with tincture of galls.

This experiment was repeated with fixed air from different combinations. As also by passing this air through a vessel of pearl-ash, to arrest any acid which might escape from the effervescing mixtures. But the solutions of iron, in all the trials, appeared to be exactly similar, except some trifling difference in taste and smell.

EXPERIMENT IV.

A bottle, with the like quantity of steel-filings and distilled water as in Experiment the First, remained
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in a room many weeks; yet although it was frequently shaken, and had an ochrous sediment, it gave no colour with tincture of galls,

EXPERIMENT V.

A single grain of iron in solution*, diluted with a pint of distilled water, changed to a deep blue purple, with the tincture of galls. Half a pint of the like mixture, exposed many days in a wide-mouthed glass covered loosely with paper, let fall a slight precipitation; but its property of tinging with galls was not sensibly diminished. The same quantity being boiled four or five minutes, in a Florence flask, became turbid, and deposited a small portion of an ochrous sediment. The tincture of galls, nevertheless, gave as deep a colour to the clear liquor, as it would have done before boiling.

The foregoing experiments seem to prove, that iron remains quite unaffected by pure water, but may easily be dissolved in it on the addition of fixed air; and that in whatsoever manner this air is generated, the event will appear the same. The last experiment shews, that where iron is suspended in water, by an acid, neither exposure nor boiling will destroy its property of tinging with galls; which is the reverse of what we find to be the case with many ferruginous waters. Experiment the Second more particularly

* Iron-filings were dissolved in diluted oil of vitriol to saturation; and, by experiment, one grain of the metal, with about two grains and a quarter of the acid, were found to be contained in sixty-eight grains of the solution.

teaches, that the iron, dissolved in water by fixed air, is at least equal in quantity to what is commonly ascribed to most chalybeate springs: that this air, by which the metal is held in solution, is similar to that elastic vapour, so often mentioned by writers on these subjects; which cannot be wholly retained by the closest corking, but, gradually escaping, suffers the ochrous matter to subside. And that fixed air has greater affinity with alcalies than with iron, because addition of alkaline substances, not saturated with fixed air, will disengage the metal, while such as are charged with this principle produce no alteration.

These conclusions seemed to account for many particulars relating to medicated springs; but as all my trials had been made with iron in its metallic state, which is rarely found in nature, it was necessary to repeat them upon this mineral in the state of ore. I proceeded, therefore, to different experiments upon various ores; but did not find any of them to answer my expectation, except what is called iron sand ore, which seems to contain a perfect iron.

This, at first, offered a material objection to my former inference. But, upon a little consideration, it occurred, that waters, being first charged with pyritical matter, might afterwards have their acid neutralized with alkaline or calcarious substances, and the iron yet remain suspended by air generated in the saturation. And I was the more ready to adopt this opinion, as it would explain, very naturally, the application of fixed air to this business of solution; which, I confess, had hitherto been to me somewhat difficult to account for. It was necessary, however, to examine the truth of this theory, by
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the test of experiment, which I did in the following manner.

EXPERIMENT VI.

To a pint of distilled water, mixed with one grain of iron dissolved (as in Experiment the Fifth), were added forty grains of soap lye.

And to two ounces of lime-water, diluted with fourteen ounces of distilled water, was added a grain of iron in solution.

In both cases the point of saturation was intended. The two mixtures instantly turned green, grew turbid, and let fall sediments of the same colour. The liquors, being filtered, gave no tinge with tincture of galls.

EXPERIMENT VII.

A quart of water was mixed with two grains of iron, as before.

To one moiety, three grains of salt of tartar in solution was instilled. It first appeared green, soon changed yellow, and gave an orange-coloured precipitate.

To another portion two grains of powdered chalk being added, it presently became straw-coloured, and after continuing nine hours in a well-corked phial, was yet turbid, with a yellow sediment.

The waters being both filtered, part of each changed to a purple, with tincture of galls. The remainder being boiled, a farther precipitation ensued, and the clear liquor no longer produced any alteration with galls.

This experiment being repeated with magnesia, and with the earth of alum, shewed no sensible difference.

The quantity of iron, left dissolved in the liquor, was found in proportion to the volume of air generated during the saturation.

If the quantity of alcali or absorbent earth was insufficient to saturate the acid, part of the iron would remain in the water after boiling. All which were discoverable by the tincture of galls.

EXPERIMENT VIII.

To a pint of distilled water, being saturated with fixed air, and containing four grains of chalk, was added a grain of iron in solution; the mixture continued pellucid.

Another grain of dissolved iron was diluted with a like quantity of water, previously saturated with air from a solution of pearl-ash in vitriolic acid: eight grains of salt of tartar, crystallized with fixed air, and dissolved, were added to this mixture: a slight degree of cloudiness ensued, but disappeared on shaking; after which the liquor much resembled Spa-water; only it tasted stronger of the iron. The quantity of chalk, and alkaline salt, in this experiment, was more than sufficient to neutralize the acid.

Both the above mixtures, on exposure, became turbid, threw up pellicles, deposited ferruginous sediments, and lost their power of striking a purple with galls.

Solutions of iron, and of its ores, in the marine and nitrous acids, as also pyrites dissolved in rain-water, were substituted, by turns, instead of the original iron in vitriolic acid; and they all gave a purple colour with tincture of galls.

The trials were repeated with lime-stone, marble, island crystal, osteocolla, and magnesia, in lieu of chalk; and with volatile and mineral alkali, replete with air, in the room of salt of tartar; but no material difference was observed.

The success of these experiments compleatly answered my expectations. They satisfied me, that any acid holding iron dissolved, and diluted with water, might not only be neutralized, but the water charged with an excess of alkaline or earthy matter, without precipitating the metal; and that the solvent, in these cases, could be no other than *Fixed Air*. Since the iron remained in solution, only where this principle originally abounded in the water, or was afterwards generated in the saturation.

Thus much being determined, it seemed easy to apply the discovery to the more perfect analyzation of some waters; and to the re-production of others, by art, which should exactly resemble those of natural medicated springs. This is a task I should probably have undertaken at leisure; had I not been informed, that Dr. Watson, junior, by whose conversation my thoughts were first led to the subject, is already engaged in something of this sort. This gentleman saw many of the foregoing experiments repeated; and, as he is since gone to the German Spa, I dare say, his abilities and application will sufficiently improve the
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the opportunity: I shall gladly, therefore, wait the result of his enquiries.

To conclude then: It appears to me highly probable, that fixed air is generally necessary to the impregnation of mineral springs. That by the right knowledge of this principle, we may now solve most difficulties that have arisen on this subject; and very possibly be able, hereafter, to imitate nature, in the formation of medicated waters. Whether my conclusions are well founded, I with pleasure refer to your candid decision. And am,

S I R,

with great respect,

Your much obliged,

humble servant,

T. Lane.

Received June 19, 1769.

XXXI. *Account of several Phænomena observed during the Ingress of Venus into the Solar Disc. By the Reverend W. Hirst, F. R. S. in a Letter to the Astronomer Royal.*

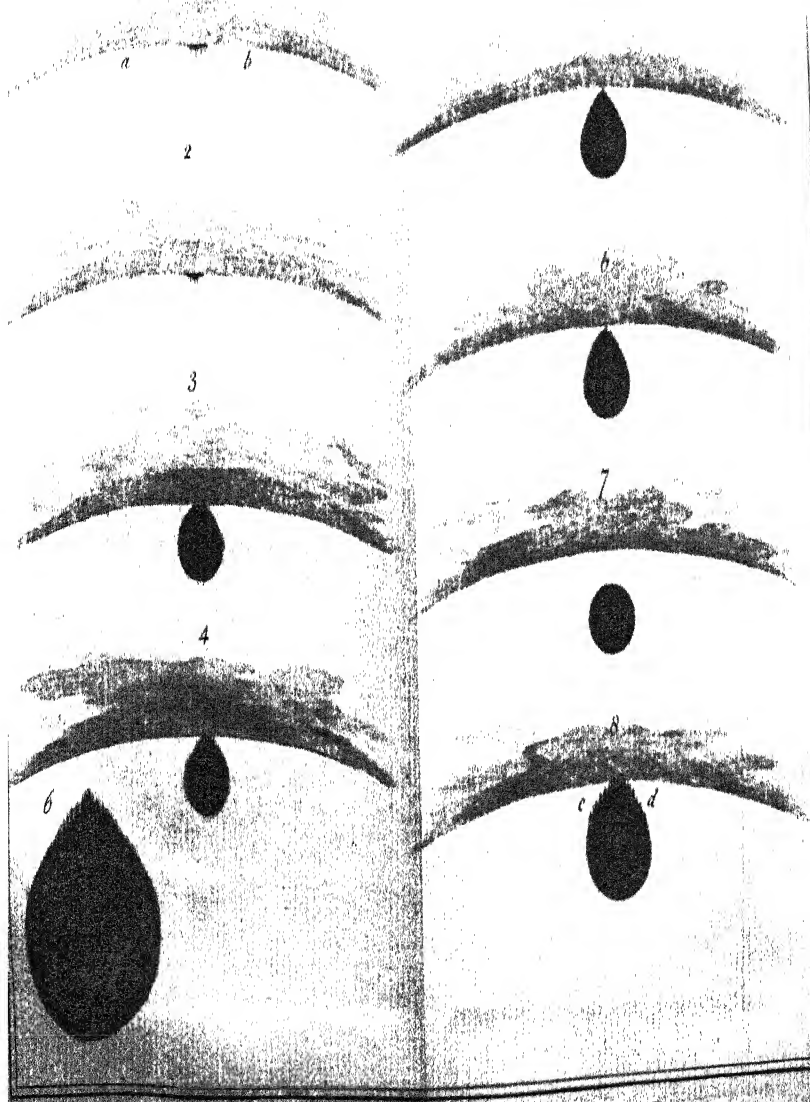
Inner Temple, June 12, 1769.

DEAR SIR,

Read Nov. 16,
1769.

AGREEABLY to your desire, I now send you a particular account of all the phænomena I observed during the ingress of Venus into the solar disc ; but, before I do this, I look upon myself as obliged in gratitude to return you many thanks for the kind manner in which you accommodated me with the apparatus necessary for the observation. The telescope I used, as you well know, belonged to Mr. Dunn, and was a reflector two feet in length, and magnified 55 times. Expecting the planet to enter the solar disc at or near the zenith, I kept my eye constantly fixed at that part of the Sun a considerable time before the beginning of the transit. The first intimation which I had of the near approach of the planet, was by the sudden appearance of a violent corruscation, ebullition,

Fig. 1.



tion, or agitation of the upper edge of the Sun, as in TAB. X. and XI. fig. 1. when I called out to my good friend, governour Vansittart, who was so kind to give himself the trouble of taking the time for me, and desired him to take care. I had not taken this precaution above five or six seconds, when I plainly saw a black notch breaking in upon the Sun's limb, and which seemed a portion of a much less sphere than that of Venus, as in fig. 2. Instantly I desired Mr. Vansittart, by the word *Now*, to mark the time, which was $11^h 57' 35''$ sidereal time, by Dr. Halley's little clock, belonging to the royal observatory. The last-mentioned time, allowing for the necessary corrections, and reduced to apparent time, is $7^h 11' 11''$.

As I imagined, from the instructions of Dr. Halley, that the precise and accurate time of observing the internal contact is, when the thread of light should break in between the concave edge of the Sun and the convex edge of Venus, as in fig. 6. I waited till that particular period, which was when Dr. Halley's clock marked $12^h 15' 45''$, sidereal time, or $7^h 29' 18''$, apparent time, the difference being $18' 7''$ of apparent time.

The same phenomenon of a protuberance, which I observed at Madras, in 1761, *at both internal contacts, I observed again at this last transit: at both times, the protuberance of the upper edge of Venus diminished nearly to a point before the thread of light between the concave edge of the Sun, and the convex edge, was perfected, when the protuberance instantaneously

* Philos. Transf. vol. LII. part i. 1761, p. 396.

broke off from the upper edge of the Sun, but Venus did not assume its circular form till it had descended into the solar disc, at least to the distance, by estimation, from the upper edge of the Sun, as described in fig. 7.

Although at the time of the contacts the atmosphere was remarkably clear, yet, as the Sun descended towards the horizon, the atmosphere grew more and more hazey, so that the edge of the Sun, as well as the edge of the Planet, began to grow more and more tremulous, and caused the Planet to assume, in appearance, different configurations, resembling sometimes a prolate and sometimes an oblate spheroid, till we lost sight of the Sun by its being intercepted by a dark cloud, or rather fog-bank, some time before the calculated time of Sun-set. Although these last phænomena are entirely optical deceptions, owing to the state of the air at that particular time, or to horizontal vapours, yet I beg it may be here very carefully remarked, that, at the times of the contacts, the air, as you can bear me witness, was perfectly clear and favourable, so that our observations were then certain, and not subjected to any fallacy in vision. The following circumstance is a proof of this assertion. The first warning which I had of the near approach of Venus to the Sun's external edge was, as I have before said, by the sudden appearance of a violent corruption, ebullition, or agitation of the upper edge of the Sun, five or six seconds before the edge of Venus broke in upon the Sun; where alone I observed the violent agitation, the edge on each side remaining perfectly quiescent, as *a, b*, fig. 1. If this appearance had been owing to the state of *our* atmosphere only,

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then would the edge of the Sun be universally fluctuating, or trembling; but as this was not the case, the undulation must be imputed to some other cause, not improbably to an atmosphere about Venus. I am the more minute on this circumstance, because you yourself, Sir, in a late * publication, have taken notice, that when I took the observation of the transit of Venus at Madras, in the year 1761, I saw *a kind of penumbra or dusky shade, which preceded the first external contact two or three seconds of time, and was so remarkable, that I was thereby assured the contact was approaching, which happened accordingly.*

Upon your foregoing paragraph I must beg leave to make the following remark, that in the transit of this present year I did not take notice of the same phenomenon as I did of the transit of Venus in India, in the year 1761; but I must here again insist upon it, that such penumbra or dusky shade I then actually saw, but I do not recollect I then saw the least undulation, ebullition, or corruscation, as happened in the transit of this present year. Yet both phenomena were conducive to the same purpose, and served to give me notice of the near approach of the planet Venus to the solar disc, the event, in both instances, justifying the presage; and both appearances might be the consequences of the same cause; which cause, as I have before observed, might be nothing less than the atmosphere of Venus. I say *might be*, for I would not be understood to assert here any

* Instructions relative to the observation of the ensuing transit of the planet Venus over the Sun's disc, on the 3d of June 1769. By the Rev. Nevil Maskelyne, Astronomer Royal, p. 32.

thing dogmatical, preserving at this time the same diffidence in expression as I made use of when I observed the transit of Venus in India, where I was apprehensive, that *to be able to discern an atmosphere about a planet at so great a distance as Venus may be regarded as chimerical*: yet I may venture to say, that my observation of the transit of the present year seems to corroborate my assertion, in the account of the transit observed in India, in 1761; however, I shall not here peremptorily assign the cause, leaving such remarks to be made by others.

On my return from India, I was glad to find I was not particular in remarking the strange phenomenon of the oblongation of the orb of Venus at the time of both the internal contacts. It was with sensible pleasure I have seen, in the † Philosophical Transactions, that four astronomers at Upsal, in Sweden, as well as ‡ Mr. Dunn, in England, took notice of the same or similar circumstances. The appearance of this protuberance or ligament must now be universally confirmed, especially by all observers of the transit of the present year, at least by all such who have viewed it through telescopes of sufficient magnifying powers, and who have sense enough to believe their own eyes, or candour enough to embrace and acknowledge conviction, *malgré* all prejudice and preconceived opinion.

Fig. 1. represents the first presage I had of the approach of Venus to the Sun's external disc. Fig. 2. is the appearance of the black notch, when I noted

* Philof. Transf. vol. LII. part i. 1761, p. 396.

† Ibid. p. 227. and vol. LVI. 1766, p. 72.

‡ Ibid. p. 184.

the time of the first external contact. Fig. 3. is the body of the planet within the solar disc, adhering to the Sun's upper edge, the thread of light not yet formed. Fig. 4. the protuberance forming, and the undulation at, *c, d*, very violent, better seen in fig. 8. Fig. 5. the undulation decreasing, and the protuberance forming itself into a point, the luminous filaments darting between the edges of the Sun and the planet. Fig. 6. the luminous filaments cease to move, and the upper edge of the planet is well defined. Its whole orb more opaque, but not yet divested of its oval appearance, the thread of light at *c, d*, is formed; and at this period I marked down the time of the internal contact. Fig. 7. the planet is restored to its circular figure.

I shall conclude this account with a few remarks I think it necessary to make on the manner in which my observation of the transit in India, in 1761, was inserted in the Philosophical Transactions. I am very much grieved, that the observations of the equal altitudes and meridional transits for regulating my time-keeper were altogether omitted. Had this not been the case, every one might have judged of the care and pains I took in that distant part of the world, as well in making several of my instruments myself, as in using them when made. What degree of confidence was to be given to my observations might be easily seen. Monsieur Pingré* would have had no occasion to lament that I did not acquaint *the world in what manner I observed the equal altitudes to determine the passage either of a fixed star, or of the Sun over the*

* Philos. Trans. vol. LIV. 1764, p. 156.

meridian, &c. If these particulars had been inserted, there would have been no occasion likewise to alter my numbers respecting the periods of the transit, since every astronomer, from the previous observations, might draw his own conclusions. Observations ought not to be rejected or stifled because they do not entirely suit any adopted system, or favourite parallaxic angle. If I declared I saw an oblongation of the planet Venus, it ought not to be discredited because another did not see it. If I gave reasons for suggesting that Venus had an atmosphere, but had not a satellite, the report should have been impartially stated, though another should be of a contrary opinion.

I shall beg leave to extract the following paragraph from my original letter from India to Lord Macclesfield, on the subject of the transit of Venus, in the year 1761, which, for what good reason I know not, was suppressed, and had not the honour of a place in the Philosophical Transactions; but which I am the more desirous should now be inserted, as it tends to elucidate a matter of fact, and to render indisputable an astronomical truth, only to be established by those who had the opportunity of seeing as I did, the entire passage of Venus over the solar disc. The paragraph is as follows:

“ Looking over the Philosophical Transactions,
 “ some time before the transit, I found Mr. Short
 “ had observed a small star near Venus, which had
 “ the same phasis as that planet. This gave suspicion
 “ that Venus was attended with a satellite. A cor-
 “ roborating circumstance was added, *viz*, M. Cas-
 “ sini, in his *Elements d'Astronomie*, mentions a like
 “ observation. This I regarded as a favourable
 “ opportunity,

“ opportunity, concluding, that if Venus had a satellite, it must be seen at its transit over the Sun’s disc ;
 “ accordingly, I gave notice of this to Captain Barker,
 “ of the Company’s Artillery” (now Colonel Sir Robert Barker), “ who took the observation at Pondicherry, I also mentioned it to the Jesuits, who
 “ observed at the Great Mount, about $7\frac{1}{2}$ miles S.
 “ 50° W. of Madras, but neither of them saw any
 “ appearance in the least like a satellite. I also spoke
 “ of it to Governour Pigot” (now Lord Pigot) “ and
 “ Mr. Call, who with myself saw not the least speck
 “ attending that planet; whence we may now venture to affirm, *That Venus has not a Satellite.*”

I am,

with great respect,

DEAR SIR,

Your affectionate, humble servant,

W. Hirst.

XXXII. *Observations made at Leicester on the Transit of Venus over the Sun, June 3, 1769. By the Reverend Mr. Ludlam, Vicar of Norton, near Leicester.*

Read Nov. 16, 1769. **T**HE telescope, used for viewing the planet, was made by Mr. Dollond, with a triple object glass of $33\frac{1}{3}$ inches focal distance, and was found by experiment to magnify 54 times. The clock was firmly fixed; its pendulum rod was made of wood. The transit telescope was not accurately adjusted either to the meridian or horizon, but the transits of the Sun and of η Bootis registered below are sufficient to show the rate of the going of the clock, and the corresponding altitudes of the Sun, its error a few days before the transit of the planet; whence the necessary reduction of the time then shewn by the clock to apparent time may be easily derived.

Observations for examining the clock

TRANSITS.							Object
Day of the month, 1769.	Time by the Clock.					Third Wire	
	First Wire.	Passage over Meridian.					
	' "	h	' "		' "		
May 28	17 12 Clouds Clouds	IX 18 0 Clouds XXIII 59 1	18 47½ Clouds 59 50	n Bootis Sun			
29	13 19½	IX 14 7½	14 54	n Bootis			
31	56 29 58 46	XXIII 57 17½ 59 35	58 6½ 0 23½	Sun			
June 1	56 39 58 56	XXIII 57 28½ 59 46	58 17 0 34	Sun			
2	57 43½	VIII 58 32	59 19	n Bootis			

Corresponding altitudes of the Sun, taken by reflection from water, with an Hadley's quadrant of six inches radius.

Sun's double alt. 79° 54'

May 29, 1769

	Time by the Clock		
	Eastern Az.	Western Az.	Meridian
	h ' "		
<i>Up. limb</i>	VIII 28 33	III 27 39	XI 58 6
<i>Center</i>	30 17 $\frac{1}{2}$	25 51 $\frac{1}{2}$	4 $\frac{1}{2}$
<i>Low. limb</i>	32 8	23 57 $\frac{1}{2}$	2 $\frac{1}{2}$
Mean			XI 58 4 $\frac{1}{2}$

Sun's

Sun's double alt. $82^{\circ} 55'$

May 29, 1769

	Time by the Clock								
	Eastern Az.			Western Az.			Meridian		
	h	'	"	h	'	"	h	'	"
<i>Up. limb</i>	VIII	39	0	III	17	8	XI	58	4
<i>Center</i>		40	$51\frac{1}{2}$		15	18			$4\frac{3}{4}$
<i>Low. limb</i>		42	$42\frac{1}{2}$		13	25			$3\frac{1}{4}$
Mean							XI	58	$4\frac{1}{8}$
Mean of both sets				h	'		XI	58	$4\frac{7}{16}$
Cor ⁿ for the interv.				7	0				$7\frac{5}{8}$
Passage over meridian							XI	57	$56\frac{5}{8}$
Equat. of time								3	3
Clock faster than mean time									$59\frac{2}{3}$

Sun's double alt. $96^{\circ} 58'$

June 2, 1769

	Time by the Clock.								
	Eastern Az.			Western Az.			Meridian		
	h	'	"	h	'	"	h	'	"
<i>Up. limb</i>	IX	27	50	II	29	44	XI	58	47
<i>Center</i>		29	57		27	41			49
<i>Low. limb</i>		32	$1\frac{1}{2}$		25	36			$48\frac{3}{4}$
Mean				h	'		XI	58	$48,25$
Cor ⁿ for interv.				5	0				$5,65$
Passage over meridian							XI	58	$42,6$
Equat. of time								2	$28,4$
Clock faster than mean time								1	11

If we suppose the clock to be $1' 11''$ faster than mean time, on June the second at noon; and to gain at the rate of $2\frac{1}{2}$ seconds in a day, then at the time of the transit of the planet it was one minute and one second slower than apparent time.

At VII^h 6' 0'', according to the time shewn by the clock, a small indenture appeared on the Sun's limb; the increase of it at VII^h 6' 14'', shewed plainly that it was made by the expected planet.

The

The internal contact was first noted at vii^h 23' 56''; at vii^h 24' 8'', the divided part of the Sun's limb seemed wholly united.

The edge both of the Sun and Planet were in a continual tremor; at the internal contact the limb of the Sun seemed, for several seconds, to be alternately united and again separated by a kind of shootings of the Planet.

These observations, reduced to apparent time, give the external contact at vii^h 7' 1'', the internal contact at vii^h 25' 9'', the duration 18' 8''.

The solar eclipse was observed by the same clock and telescope. It was manifestly begun at xviii^h 34' 26'', according to the time shewn by the clock. The ending was exactly noted at xx^h 20' 8''. The Sun's limb appeared very well defined all the morning. These observations, reduced to solar time, make the beginning of the eclipse at xviii^h 35' 21'', the end at xx^h 21' 2'', the duration 1^h 45' 41''.

Observations made at Leicester, with an Hadley's quadrant, of six inches radius, for determining the latitude of the place.

1769			°	'
April 27	Sun's diameter on quadrantal arch			32
	on arch of excess			33
	repeated on quad. arch			32
	on arch of excess			32
	Sum of the meridian altitude of the Sun's upper limb, and its depression, when reflected by water	103	20½	
	of the lower limb	102	18	
29	Sun's diameter on quadrantal arch			32½
				April

1769									
April 29	Sun's diameter on arch of excess							32	
	repeated on quadrantal arch							33	
	on arch of excess							32+	
	Merid. alt. and depression of Sun's upper limb,								
	reflected by water					104		36	
	of the lower limb					103		31	
June 11	Sun's diameter on quadrantal arch							33	
	on arch of excess							30	
	repeated on quad. arch							33	
	on arch of excess							30	
	Merid. alt. and depression of Sun's upper limb,								
	reflected by water					121		36½	
	of the lower limb					120		33	
15	Sun's diameter on quad. arch							33½	
	on arch of excess							30	
	repeated on quad. arch							33½	
	on arch of excess							30	
	Merid. alt. and depress. of Sun's upper limb, re-								
	flected by water					122		2½	
	of the lower limb					121		59½	

		0	1	17
The latitude of Leicester, deduced from the ob-				
servations of April 27, is	52	36	21	
of April 29,	52	37	3	
of June 11,	52	37	35	
of June 15,	52	37	12	
Mean of these four observations	52	37	3	
The latitude of Market Harborough, in Lei-				
cestershire, from the mean of several accurate				
observations of the Sun's image, projected in-				
to a dark room, by S. Rouse	52	28	30	

XXXIII. *A Letter from John Hope, M.D. F. R. S. Professor of Physic and Botany in the University of Edinburgh, to William Watson, M.D. F. R. S. on a rare Plant found in the Isle of Skye.*

S I R,

Read May 4, 1769. I SEND you, inclosed, a description of a rare plant, with a print of it of the natural size.

It was found, September 1768, in a small lake in the island of Skye, by James Robertson*, whom I had sent there in search of new or rare plants. The whole of it, except the head and top of the stalk, was under the surface of the water. Wherever the water was shallow, the bottom of the lake was covered with this plant, whose roots were so closely interwoven, that in some places large patches were torn up by the agitation of the waters, or other violence, and found floating on the surface, matted together.

The plant, when seen without its flowering stem, resembles somewhat the *Calamaria Dill. Hist.*

* Mr. James Robertson is an eleve of mine, and has been employed by the commissioners of the annexed estates to make a botanical survey of the distant parts of Scotland.

Musc. Tab. 80. At first sight I fancied it to be the same, and that the *Calamaria* had not been found with its flowering stem : more careful comparison convinced me they were different plants.

Although it differs, in many circumstances, from the generic characters of the *Eriocaulon*, yet I am inclined to think it is the *Eriocaulon decangulare*, which has never yet been described, or properly figured.

I have the honour of being,

DEAR SIR,

Your most obedient servant,

Edinburgh, 10 April,
1769.

John Hope.

ERIOCAULON DECANGULARE.

RADIX perennis, solida, interne alba, ex cujus parte inferiore oriuntur plurimæ radiculae teretes, simplicissimæ, pennæ passerinæ crassitudinis, albæ, fere pellucidæ, per totam longitudinem dissepimentis opacis, transversis, sive articulationibus interceptæ, in centro harum per totam longitudinem est linea quædam opaca. Extremitates describere non audeo, quia in singulis speciminibus mihi oblatis, omnes radiculae ruptæ fuerunt. Sapor radicularum primo insipidus est, levi postea cum acrimoniâ.

FOLIA RADICALIA, ex parte superiore radicis oriuntur plurima, sub-erecta, similia quod ad figuram externam, et simili modo disposita ac in *Agave Americana*: sesqui-unciam longa, lata basi, margine integerrimâ, sensim attenuantur in apicem acutum, pagina inferiore convexa; superiore concava, nervosa: et spatia, his nervis longitudinalibus definita, iterum dividuntur dissepimentis transversis, singulis singulis propriis, ut in figura.

TRUNCUS scapus erectus; e centro foliorum oritur, vagina tenui membranacea, apice bifido, duas uncias longa, respectu nervorum et dissepimentorum foliis simili, inclusus, subteres, fistulis septem in ambitu conflatus, supra vaginam nudus, contortus.

FRUCTIFICATIO androgyna in capitulo terminali, globofo, imbricato.

CALYX communis, squamis plurimis, nigris, subrotundis, concavis, membranaceis; parte superiore ciliatis; duæ sunt series florum foemineorum in radio.

FLOS FOEMINEUS IN RADIO.

Squama, ovata, nigra, superne ciliata sensim definens in unguem brevem, externe adstat singulis flosculis.

CAL. PROP. perianthium diphyllum, foliolis ovatis, concavis, nigris, superne ciliatis, in unguem angustam sensim definientibus. Not. cilia squamarum sunt alba.

COROLLA dipetala, petalis albis, oblongis, concavis, sensim definientibus in ungues angustos, apice et dorso ciliatis, et macula nigra in medio fere laminæ notatis.

PISTILLUM germen compresso-subrotundum, stylus brevis, stygmata duo longa, filiformia.

PERICARPIUM capsula compresso-subrotunda, nigra, bilocularis.

SEMEN in singulis loculis unicum, læve, dissepimento affixum citrini coloris, ad apicem umbilicatum.

FLORES MASCULI IN DISCO PLURES.

Squama adstat flosculis masculis, ut in foemineo flore.

CALIX perianthium diphyllum foliolis cuneiformibus, concavis, ciliatis.

COROLLA

COROLIA monopetala infundibuliformis, ore
bilabiato, fimbriato.

STAMINA filamenta (quatuor?*) filiformia,
longitudine, corollæ, vel longiora.

Antheræ nigræ, oblongæ.

Edinæ, Apr.
1769.

J. Hope, *M. & Bot. P.*

* Staminum numerum definire vix audeo.

EXPLICATIO TABULÆ XII.

Fig. 1. Plantæ ut naturaliter crescunt.

Fig. 2. Planta unica seorsim exhibita, ut apparuit
aqua immerfa et inter lucem et oculum visa.

- a Capitulum magnitudine auctum.
- b Scapi fragmentum magnitudine auctum.
- c Fibrilla radice magnitudine aucta.
- d Folium transverse sectum magnitudine auctum.

PARTES FRUCTIFICATIONIS.

- | | | |
|-----------------|---|--|
| Flos foemineus. | { | A Squama calycis communis. |
| | | B Squama quæ singulis flosculis foemineis adstat. |
| | | C Foliola duo perianthii proprii flosculum foemineum tegentia. |
| | | DD Eadem foliola perianthii proprii e situ naturali remota. |
| | | E Pars germinis. |
| | | F Petala |
| | | GG Eadem petala e situ naturali remota. |
| | | H Germen. |
| | | I Stylus. |
| | | K Stygmata. |
| Flos masculus. | { | L Pericarpium transverse sectum. |
| | | M Semina duo. |
| | | N Squama quæ singulis flosculis masculis adstat. |
| | | O Foliola duo Perianthii proprii. |
| | | P Petalum. |
| | | R Petalum apertum ut insertio staminum appareat. |
| | | S Petalum staminibus remotis. |
| T Stamen. | | |

XXXIV. *Astronomical Observations made by Samuel Holland, Esquire, Surveyor-General of Lands for the Northern District of North-America; and Others of his Party. Communicated by the Astronomer Royal.*

Obs. of Lat.

MARCH 8, 1769, observed by Samuel Holland, Esquire, at his house, bearing south, 56° west from Quebec, distance from the castle of St. Lewis $2\frac{1}{2}$ miles, with Bird's astronomical quadrant, the latitude, viz.

Zenith angle of the Sun's upper limb	51	4	0
Deduct for the Sun's southern declination		4	34 31

Add the Sun's semi-diameter	46	29	29
Ditto refraction		16	9
		1	42

North latitude by observation	46	47	20
-------------------------------	----	----	----

Obs. of Long. March 11, 1769, observed by the same at the same place, with Dollond's refracting telescope, an immersion of the first satellite of Jupiter, at 15 hours, and 45 seconds, mean or equal time.

Obs.

Obf. of Lat. March 19, 1769, observed by the same at the same place, with Bird's astronomical quadrant, the latitude, viz.

	°	'	"
Zenith angle of the Sun's upper limb	46	45	2
Deduct for the Sun's southern declination	} 14 43		
	<hr/>		
Add the Sun's semi-diameter	46	30	20
Ditto refraction		16	6
	<hr/>		
North latitude by observation	46	47	16

Obf. of Lat. March 20, 1769, observed by the same, at the same place, with the same instrument, the latitude, videlicet,

	°	'	"
Zenith angle of the Sun's upper limb	46	21	16
Add the Sun's northern declination		8	58
Ditto the Sun's semi-diameter		16	5½
Ditto the refraction		1	0
	<hr/>		
North latitude by observation	46	47	19½

N. B. Six more observations of the latitude have been taken. The mean result of the whole is

46 47 15

Obf. of Long. April 3, 1769, observed by the same, at the same place, with Dollond's refracting telescope, an immersion of the first satellite of Jupiter, at 15 hours, 10 minutes, and 22 seconds, mean or equal time.

Obf. of Long. April 18, 1769, observed by the same, at the same place, with the same instrument, an immersion of the second satellite of Jupiter, at 12 hours, 39 minutes, and 36 seconds, mean or equal time.

Obf. of Long. April 19, 1769, observed by the same, at the same place, with the same instrument, an immersion of the first satellite of Jupiter, at 13 hours, 26 minutes, and 27 seconds, mean or equal time.

Obf.

- Obs. of Long.** May 28, 1769, observed by the same, at the same place, with the same instrument, an emersion of the first satellite of Jupiter, at 14 hours, 2 minutes, and 40 seconds, mean or equal time. Also, observed by the same, at the same place, with the same instrument, a superior conjunction of the fourth satellite of Jupiter, at 11 hours, 14 minutes, and 17 seconds, mean or equal time; and it entirely disappeared at 11 hours, 24 minutes, and 3 seconds, mean or equal time.
- Obs. of Trans.** June 3, 1769, observed, by the same, at the same place, with the same instrument, the Transit of Venus, as follows: at 2 hours, 28 minutes, and $1\frac{1}{2}$ seconds, perceived a luminous point on the lower part of the Sun's limb, by appearance; and, in the same place, $1\frac{1}{2}$ seconds afterwards, the first external contact was formed, which rectified as the clock or time-piece of Graham was 15 seconds too fast at the time of observation (as proved by equal altitudes of the Sun taken with Bird's astronomical quadrant, on the 1st, 2d, 4th, and 5th instant) the equal or mean time of observing the first external contact will be at 2 hours, 27 minutes, and 48 seconds. Mr. St. Germain, of the seminary of Quebec, observed the same contact, at the same instant, with Short's 2 feet reflecting telescope. Clouds, intervening, prevented the observation of the first internal contact: but at 6 o'clock the Planet might be seen with the naked eye on the Sun's disc, through the haziness of the atmosphere.
- Obs. of Long.** June 6, 1769, observed by the same, at the same place, with the same instrument, an emersion of the first satellite of Jupiter, at 10 hours, 26 minutes, and 22 seconds, mean or equal time.

Obs. of Lat. January 2, 1768, observed by Ensign George Sproule, of the 59th regiment of foot, on the south point, at the entrance of the basin of Gaspée, with Hadley's quadrant, and an artificial horizon, the latitude, viz.

Double angle of the meridian }
altitude of the Sun's center } 36 38 0

Apparent altitude of the Sun's center 18 19 0
Refraction 2 41

True altitude of the Sun's center 18 16 19
90 0 0

Sun's zenith distance 71 43 41
Sun's declination reduced to the }
meridian of Gaspée } 22 56 10

North latitude by observation 48 47 31

Obs. of Lat. May 9, 1768, observed by the same, at the same place, with the same instrument, and an artificial horizon, the latitude, videlicet;

Double angle of the Sun's lower }
limb, meridian altitude } 117 6 0
Add for adjusting the quadrant, er- }
ror to the right, } 2 10

117 8 10

Apparent altitude of the Sun's }
lower limb } 58 34 5
Add the Sun's semi-diameter 15 53

Apparent altitude of the Sun's center 58 49 58
Deduct for Refraction 33

True altitude of the Sun's center 58 49 25
90 0 0

Sun's zenith distance 31 10 35
Add the Sun's declination, reduced }
to the meridian of Gaspée } 17 36 56

North latitude by observation 48 47 31

Obs.

Obs. of Lat. May 15, 1768, observed by the same, at the same place, with the same instrument, and an artificial horizon, the latitude, viz.

Double angle of the Sun's upper limb, meridian altitude	121 10 00
Subtract for adjusting the quadrant error to the left.	35

121 9 25

Apparent altitude of the Sun's upper limb	60 34 42
Subtract the Sun's semi-diameter	15 51

Apparent altitude of the Sun's center	60 18 51
Subtract for refraction	31

True altitude of the Sun's center	60 18 20
	90 0 0

Sun's zenith distance	29 41 40
Add Sun's declination reduced to the meridian of Gaspée	19 5 50

North latitude by observation	48 47 30
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N.B. There were 12 more observations made of the latitude, by the same person; but these are judged sufficient to shew his manner of operation; but the result of the whole 15 make the place of observation 48° 47' 32" north latitude.

Obs. of Long. January 29, 1768, observed by the same person, at the same place, with Short's two feet reflecting telescope, an immersion of the first satellite of Jupiter, at 14 hours, 11 minutes, and 33 seconds, mean or equal time.

Obs. of Long. March 15, 1768, observed by the same person, at the same place, with the same instrument, an immersion of the first satellite of Jupiter, at 14 hours,

hours, 29 minutes, and 38 seconds, equal or mean time.

Obs. of Long. March 16, 1768, observed by the same, at the same place, with the same instrument, an immersion of the second satellite of Jupiter, at 12 hours, 7 minutes, and 16 seconds, equal or mean time.

Obs. of Long. March 16, 1768, observed an immersion of the third satellite of Jupiter, at 13 hours, 38 minutes, and 18 seconds, equal or mean time; by the same person, with the same instrument, at the same place.

Obs. of Long. April 9, 1768, observed by the same person, at the same place, with the same instrument, an emersion of the first satellite of Jupiter, at 11 hours, 19 minutes, and 24 seconds, equal or mean time.

Obs. of Long. April 10, 1768, observed by the same person, at the same place, with the same instrument, an emersion of the second satellite of Jupiter, at 11 hours, 38 minutes, and 45 seconds, equal or mean time.

Obs. of Long. April 25, 1768, observed by the same person, at the same place, with the same instrument, an emersion of the first satellite of Jupiter, at 9 hours, and 37 minutes, equal or mean time.

N. B. This observation is thought to be as exact as possible, the satellite emerging totally in an instant, and the clock being truly regulated by a number of single and corresponding altitudes.

Obs. of Long. May 9, 1768, observed by the same person, at the same place, with the same instrument, an emersion of the first satellite of Jupiter, at 13 hours, 26 minutes, and 47 seconds, equal or mean time.

Obs. of Long. May 12, 1768, observed by the same person, at the same place, with the same instrument, an emersion of the second satellite of Jupiter, at 11 hours, 11 minutes, and 34 seconds, equal or mean time.

Samuel Holland.

XXXV. *Observations made on the Island
of Hammerfoft, for the Royal Society.
By Jeremiah Dixon.*

A JOURNAL of my OBSERVATIONS at
HAMMERFOST.

-
1769.
 ☉ May 7 At half past 10 A. M. anchored in Hammerfoft-Bay, near the town of Hammerfoft, on Hammerfoft-Island.
- In the afternoon went on shore, to find a proper place to observe in; but found none.
- 8 Went on shore again, to find a place; and, after much search and travel, fixed upon one: but, though the best this or the adjacent islands could afford, is very difficult of access.
- 9 Landed the house and observatory.
- 10 Digging holes for fixing the clock-post, and stand for the transit-instrument. Note, the ground so much frozen and rocky, could not finish them this day.

1769.

May 10 | The carpenters, this day, put up the house, separate from the observatory; as there was not room left by the carpenter at Greenwich for fixing the transit-instrument.

11 | Finished the digging, and fixed up the post for the clock; also put up that part of the observatory which moves round. Note, the post for the clock was 3 feet deep in the ground.

12 | Carried the instruments on shore: the carpenters working at the observatory.

13 | Fixed up the clock, and set it a-going. Note, The upper part of the bob of the pendulum was set to scratch, marked thus ω and the index to N^o 14. It vibrated on each side of the perpendicular 1^o 30'.

14 | Put up the quadrant. It was placed upon the stand provided for it. The stand stood on a large cask filled with water, very firmly fixed in the ground; and the box in the bottom of the stand filled with stones. Cloudy weather.

Placed one thermometer without doors, and the other within the observatory; also placed the barometer in a proper place.

Cloudy till 7 May 20, in the evening; when I took the following equal altitudes of the Sun.

F. May 20	Sun		West.		Sun		East.		Point on the quadran.
	L. limb.	"	Up. limb.	"	Up. limb.	"	L. limb.	"	
	9 4 42		9 11 5		21 54 18		22 0 44		} 6,00
			16 26		59 37		6 5		
	15 16½		21 44		22 4 54		11 20		
	9 41 14		9 47 46 -		21 17 32		21 24 6		} 72,00
	46 33		53 8		22 58		29 31		
	52 2—		58 23		28 10		34 50		
	10 6 1		10 12 40 +		20 52 24		20 59 10		} 74,00
	11 28		18 9½		58 0		21 4 40		
	16 58		23 42—		21 3 27		21 10 7		
☉ May 21	10 12 13		10 18 53		} These correspond to the above point 74, taken in the morning.				
	17 43		24 26						
	23 16		30 3						

From the points 72 and 74, in the above sets of observations, I find the clock on the 21st, at noon, to be 12' 53", 2 too slow; and loses at the rate of 2" per day of sidereal time.

☽ May 22 Marked a meridian. The mark S° is distant about 2½ miles, and that N° about 2 miles. The situation would not allow me to have marks nearer.

Put the clock forward, to be nearer the right ascension of the mid-heaven.

This afternoon came on a most violent storm of wind, hail, and snow, which continued till Tuesday evening.

☽ — 23 Cloudy, with snow, &c.
 ☽ — 24 Ditto. Fixed the transit-instruments.
 ☽ — 25 }
 ☽ — 26 } Cloudy, snow, &c.
 ☽ — 27 }
 ☽ — 28 }
 ☽ — 29 }

[256]

1769
May 30

Sun	West.	Sun	East.	Point.
L. limb.	Up. limb.			
h ' "	h ' "			
9 29 28	9 35 56½			} 65,00
34 48	41 17			
40 6	46 35			
49 46½	9 56 12			} 66,40
55 6	10 1 29			
10 0 21	6 49			
14 4	10 20 32			} 68,40
19 22	25 53			
24 43½	31 12			
10 38 31	45 3			} 70,40
43 53	50 27½			
49 18½	55 52			

2 ^d wire.	3 ^d or middle wire.	4 th wire.	
h ' "	' "	' "	
16 34 33½	35 19	36 4½	Sun's first east limb passed the transit. Last or west limb, passed at midnight.
---	37 35+	---	

Zen. dist. Sun's upper limb 86° 47' 50".

Sun	East.		
Up. limb.	L. limb.		
h ' "	h ' "		
23 46 5	23 52 34		} 63,00
51 28	57 58		
56 50	0 3 20		
0 10 44	0 17 22		} 61,00
16 14	22 54		
21 44	28 25		
0 31 46½	0 38 34		} 59,20
27 23	44 14½		
43 3	49 58		

1769 24 June 1	Sun Up. limb. h ' "	East. L. limb. h ' "	Sun	West.	Point.	
	53 31: 1	0 0 33½				} 57,40
	59 20	0 6 27				
	-----	12 29½				
	Very hazy.					
	1st wire.	2d wire.	3d or middle wire.	4th wire.	5th wire.	
	h ' "	' "	' "	' "	' "	
2 June 2 at noon.	4 39 39	0 40 24	41 10— 43 27	41 55	42 40	Sun's W. limb the transit. E. limb passed

Zen. dist. of the Sun's upper limb $48^{\circ} 5' 50''$.

The pendulum of the clock now vibrates $1^{\circ} 20'$ on the left hand, and 1° on the right.

This afternoon the Sun being pretty well defined, adjusted the focus of the telescope to my eye, as follows. — Short eye-piece and broad little speculum I. 20th. 25th.

0 19 12 to the right of 0 on the adjusting scale. Ditto with micrometer 0 0 I. 20th. 25th.

to the right. — Long eye-piece and narrow little speculum 0 0 7 to left of 0 I. 20th. 25th.

Long eye-piece and broad little speculum 0 18 17 to the right. — Took the means of 8 or 9 trials each.

The spot near the Sun's center was thus defined.

2 June 2	Sun L. limb. h ' "	West. Up. limb. h ' "	Sun Up. limb. h ' "	East. L. limb. h ' "	Point.	
	10 55 1	-----	-----	22 21 37		} 70,40
	11 00 26	11 7 2	22 20 26½	27 3½		
	-----	12 28	25 50½	32 26		
	11 15 50+	11 22 31—	21 53 48½	22 0 37		} 72,20
	21 21	28 3	59 23	6 9½		
	26 55	33 41:	22 4 56½	11 39		

Computing from point, the Sun's the north point of meridian at 16 34, 2 per clock.

1769 2 June 2	Sun L. limb. h ' "	West. Up. limb. h ' "	Sun Up. limb. h ' "	East. L. limb. h ' "	Point.
	11 37 3½ 42 45 48 26	43 56½ 49 42 55 29 :	21 32 1 37 49 43 33	21 39 3+ 44 45 50 25	} 74.00 °
	2d wire. h ' "	3d wire. h ' "	4th wire. h ' "		
At mid-	16 41 28 -----	43 13½ 45 20½	43 59½ -----	Sun's east or first limb passed the transit at midnight. Time ...	

Zen. dist. of the Sun's upper limb $86^{\circ} 32' 57''$. Outer arc Grand Sec. Ver.
92 1 9

	Sun Up. limb. h ' "	East. L. limb. h ' "	Sun L. limb. h ' "	West. Up. limb. h ' "	Point.	
June 2	-----	23 10 35	10 11 30::	-----	} 66,40	
June 3	23 9 26 14 38	15 52½ 21 9	16 49 22 9	10 23 17½ -----		
	23 24 20 29 37 34 53::	30 46½ 36 3½ 41 20	9 50 20 56 37 10 1 58½	9 57 44- 10 3 1 8 21	} 65,00	Computing from this point, the Sun passed the meridian at 4 ^h 46' 2", 52 per clock.
	-----	23 55 8	9 26 47	9 33 15½		
	23 54 00	-----	32 12	38 36	} 63,00	
	-----	0 5 53	37 33	-----		
	0 13 18½ 18 45½ 24 13	0 19 55 25 24 30 55	9 1 43 7 14½ 12 45 Hazy.	9 8 24 13 49 19 20	} 61,00	Computing from this point, the Sun passed the meridian at 4 ^h 46' 3", 48 per clock. The mean 4 ^h 46' 3".

Hence the clock is 11", 13 flow of sidereal time, and loses at the rate of 6", 84 per day.

By comparing the instant of time $16^h 44' 3''.42$ per clock the Sun passed the meridian under the Pole on the 2d of June, with the instant $16^h 44' 22''.05$ per clock, it passed the meridian mark per transit instrument, I find it is $18''.63$ later per transit, that is, the mark northward stands $18''.63$ too much to the east. Now by putting up another mark at the same distance as the first, so as to correspond to the second wire, and measuring the distance between this and the first mark, I find it to be 36 feet. This increased in the ratio of the zen. dist. $86^\circ 48'$ to rad. gives 36,057 feet for the space corresponding to $45''\frac{1}{2}$, the time of the Sun's passage from the second to the third or middle wire.

The reason of this increase is, that all the wires in a transit instrument, except the middle one, describe lesser circles, parallel to the middle one, which describes an azimuth.——Having got what space corresponds to $45''\frac{1}{2}$, we have, by proportion, 14.76 feet for the space, answering to $18''.63$, which the mark must be moved westward to be in the true meridian.

THE TRANSIT OF VENUS.

Time p. clock				
1769	h	'	''	
2 June 3	13	40	00	Saw the planet Venus upon the Sun about half immersed:
		43	00	Not totally immersed.
		50	00	The planet seemed to be compleatly upon the Sun, but no thread of light: this was an instantaneous view, and through a thin cloud.
				The air all this time very hazy.
	19	47	00	Saw Venus upon the Sun's limb, the 2d internal contact being past. After this, all cloudy as before.

THE ECLIPSE OF THE SUN.

Time p. clock					Micrometer		Ap. time		Value of microm.		
h	'	''	Inch.	2 ^d h	h	'	''	0	'	''	
2	3	23	3	0	20	13	48	$\frac{1}{2}$	18	51.9	The versed sine of light
		7	44	2	13	18	9		16	39.1	Ditto
		10	30	2	9	20	54.3		15	27.4	Ditto
		13	33	2	5	23	57		14	6.14	Ditto
		22	5	4	17	32	27	$\frac{1}{2}$	30	22.8	Diff. of cusps
		24	57	4	19	35	19		30	52.7	Ditto
		30	4	1	6	40	25	$\frac{1}{2}$	8	13.15	Versed sine of light
		33	37	1	4	43	57.6		7	33.6	Ditto

L 1 2
Time

L 1 2

Time

Time, clock Micrometer						Ap. time Value of microm.					
	h	'	"	inch	25th		h	'	"	inch	25th
1769											
June 3	2	38	55	1	23		49	14	7	7	23.9
		41	3	1	21		51	52	2	7	41.3
		44	8	1	7		54	26	8	8	7.9
		47	20	1	8		57	38	3	8	54.2
		50	45	1	11		22	1	2.7	9	51.7
	3	1	45	4	13		12	0	8	28	55.3
		4	5	4	10		15	5	5	28	10
	3	38	20				48	30			
				5	1	} Sun's diameter 31' 31"					
				5	1						
				5	1						
					3	Sub. for adjustment					
June 4	Zen. diff. of the Sun's lower limb at noon 48° 23'										

A Table shewing the minutes and seconds answering to the divisions of the micrometer scale.

inch	'	"	inch	'	"	25th	"
1	6	13.08	1	0	18.654	1	0.746
2	12	26.16	2	0	37.308	2	1.492
3	18	39.24	3	0	55.962	3	2.238
4	24	52.32	4	1	14.616	4	2.984
5	31	05.40	5	1	33.270	5	3.730
			10	3	6.540	10	7.460
			15	4	39.810	15	11.190
			20	6	13.080	20	14.920
						25	18.650

- June 4 Took the dip of the sea from the observatory, and found it to be nearly 21'.
Packing up the instruments.
- June 5 Got all the instruments on board the Emerald.

A Table shewing the Height of the Mercury in Fahrenheit's Thermometer, v the Observatory, and without in the Shade, three Times per Day; and the H of the Mercury in the Barometer once per Day, while at Hammerfest.

		Within observatory.			Without observatory. Barometer.			
		Morn.	Noon.	Even.	Morn.	Noon.	Even.	
1769								
May	15	—	32	—	—	32	—	27.3
	16	31	32	33	31	33	33	27.69
	17	31	36	32	31	35	31	27.69
	18	34	36	37	34	37	36	27.00
	19	34	38	39	35	39	38	27.83
	20	40	47	43	40	43	43	27.80
	21	36	36	30	36	36	30	27.70
	22	38	38	31	38	37	30	27.00
	23	36	36	35	36	36	35	27.93
	24	36	36	36	36	36	36	27.74
	25	33	34	31	33	34	31	27.60
	26	34	36	35	34	35	35	27.50
	27	33	37	37	33	37	37	27.90
	28	32	35	34	33	36	33	27.91
	29	36	39	34	36	39	33	27.74
	30	39	49	52	36	49	55	27.65
	31	50	50	46	50	50	46	27.71
June	1	43	43	40	43	43	40	27.90
	2	40	44	44	40	44	44	27.70
	3	41	42	40	41	42	40	27.60
	4	38	38	36	38	38	36	27.50

The Latitude of the Observatory is, per obs. June 2, at noon $70^{\circ} 38' 22''$
4, at noon $70^{\circ} 38' 23''$

Note, The error of the line of collimation of the quadrant did not, upon appear to be any thing worth notice.

The altitudes, northward, are so low as not to be depended upon.

The longitude of the observatory is (by comparing the observations of the ecl the Sun with those made at Greenwich),

	h	'	"	
Per 1st measurement with the micrometer	1	34	40	These observations made before the of the eclipse.
2d ditto	1	34	35	
3d ditto	1	34	44	
13th ditto	1	35	—	These observations after the middle
15th ditto	1	35	13	
End of the eclipse	1	35	10	

$1\ 34\ 55'' = 23^{\circ} 43' 45''$

East of the Royal observatory at Greenwich.

Jere. Di

XXXVI.

XXXVI. *Astronomical Observations made at the North Cape, for the Royal Society. By Mr. Bayley.*

- 1769 April 28, 29, and 30, got the observatory and dwelling-house built, and instruments on shore.
- May 1 Set up an oak plank $4\frac{1}{2}$ inches thick, and 14 inches wide. This plank was set a little more than 2 feet in the ground, and well rammed with earth and stones so that it was very steady and firm; to which I screwed the astronomical clock truly perpendicular (by which means it was independant of the observatory and its shaking by the wind) and set it going nearly with fidereal time.
- 3 Set up the transit instrument nearly in the meridian.
- 4 Examined the line of collimation of the quadrant, and found it correct.
Examined the clock, and found the pendulum to vibrate $1^{\circ}\frac{1}{2}$ on each side of nothing.

Here follow some corresponding altitudes of the Sun, from which the going of the clock is determined.

	'	"	'	"	'	"		Z. D.
6	27	13	23	34	37	42	22	Morning
	20	58	6	13	30	5	48	Afternoon
		5	57	0	14	31	23	8
		42	6	5	33	32	24	55
7	29	16:	0	38	39:	48	24:	Morning
	26	24:	5	16	59:	—	—	Afternoon
	39	20:	0	49	17:	59	44:	Morning
	16	18:	—	—	—	—	—	Afternoon
8	29	53	23			44	29	Morning
	34	0	6			19	25	Afternoon
	37	39	23	45	13	—	—	Morning
	26	16	6	18	44	—	—	Afternoon
13	26	46	21	33	0	38	59	Morning
	33	23		39	35	45	34	Afternoon
This day set up a meridian post nearly in the meridian, by help of the quadrant, at about half a mile from the observatory. Examined the pendulum of the clock, and found it to vibrate $1^{\circ}\frac{1}{2}$ on each side of nothing.								
15	—	—	1	9	25	18	54	Morning
	—	—	5	49	26	39	54	Afternoon
	9	59	1	19	46	—	—	Morning
	48	48	5	39	0	—	—	Afternoon
18	48	56	21	55	4	1	8	Morning
	55	31	22	1	42	—	—	Afternoon
20	—	—	1	36	21	46	20	Morning
	—	—	6	1	48	51	50	Afternoon
	37	3	1	47	16	—	—	Morning
	1	10	5	50	56	—	—	Afternoon

Corresponding Altitudes of the SUN.

1769

Z. D.

☉	21	-- --	h 22 6 2	12 6	Morning	Sun's upper limb	} 69,27	
		6 28	12 38	-- --	Morning	Sun's lower limb		
♀	26	At 7 ^h 22' per clock, clock stopped, but by what cause I cannot find, it not down.						

At 9^h 1st let it going again, as near as I could guess, with fidereal time.

Pendulum vibrates 1⁰ $\frac{1}{2}$ on each side of nothing.

24 June 1 Wound up the clock The pendulum vibrates 1⁰ $\frac{1}{2}$ on each side of nothing.

2	3	49 45	22 55 59	2 3	Morning	Sun's upper limb	67,40
		-- --	10 34 52	28 47	Afternoon	Sun's lower limb	
		56 23	23 2 34	-- --	Morning	Sun's upper limb	66,50
		34 28	10 28 14	-- --	Afternoon	Sun's lower limb	
		-- --	23 6 16	12 19	Morning	Sun's upper limb	63,20
		-- --	10 24 31	18 26	Afternoon	Sun's lower limb	
		6 40	23 12 53	18 51	Morning	Sun's upper limb	61,54
		24 4	10 17 52	11 56	Afternoon	Sun's lower limb	
		-- --	23 49 31	55 38	Morning	Sun's upper limb	58,42
		-- --	9 41 12	35 5	Afternoon	Sun's lower limb	
		49 56	23 56 14	02 18	Morning	Sun's upper limb	56,40
		40 47	9	28 24	Afternoon	Sun's lower limb	
		-- --	0 7 30	13 42	Morning	Sun's upper limb	53,0
		-- --	9 23 13	17 2	Afternoon	Sun's lower limb	
		7 55	14 16	20 30	Morning	Sun's upper limb	52,5
		22 48	9 16 26	10 12	Afternoon	Sun's lower limb	
		-- --	0 48 51	55 26	Morning	Sun's upper limb	50,50
		-- --	8 41 50	35 13	Afternoon	Sun's lower limb	
		49 19	0 56 2	-- --	Morning	Sun's upper limb	47,29
		-- --	8 34 37	-- --	Afternoon	Sun's lower limb	
		-- --	1 16 46	23 47	Morning	Sun's upper limb	45,11
		-- --	8 13 51	6 50	Afternoon	Sun's lower limb	
		17 14	1 24 26	-- --	Morning	Sun's upper limb	42,27
		13 23	8 6 11	-- --	Afternoon	Sun's lower limb	
		-- --	1 39 55	47 29	Morning	Sun's upper limb	39,56
		-- --	7 50 41	-- --	Afternoon	Sun's lower limb	
		41 27	1 48 11	-- --	Morning	Sun's upper limb	37,7
		-- --	7 42 29	-- --	Afternoon	Sun's lower limb	
		-- --	2 13 44	22 27	Morning	Sun's upper limb	35,8
		-- --	7 16 52	8 7	Afternoon	Sun's lower limb	
		14 18	2 23 19	-- --	Morning	Sun's upper limb	32,24
		16 18	7 7 18	-- --	Afternoon	Sun's lower limb	
		-- --	2 30 20	39 56	Morning	Sun's upper limb	30,56
		-- --	7 0 16	50 39	Afternoon	Sun's lower limb	
		30 56	2 40 52	51 8	Morning	Sun's upper limb	27,57
		59 40	6 49 45	39 24	Afternoon	Sun's lower limb	
		-- --	2 56 16	7 58	Morning	Sun's upper limb	25,33
		-- --	6 34 20	-- --	Afternoon	Sun's lower limb	
		57 2	3 9 6	-- --	Morning	Sun's upper limb	22,11
		33 33	6 21 31	-- --	Afternoon	Sun's lower limb	
24	8	at	11 15 52,5	4 mean of four observations Z. D. ☉ L. L. = 69° 26'			
♀	9	-- --	23 21 46	27 57	Morning	Sun's upper limb	67,0
		-- --	10 58 14	52 6	Afternoon	Sun's lower limb	
		22 11	23 28 26	-- --	Morning	Sun's upper limb	57,49
		57 49	10 51 39	-- --	Afternoon	Sun's lower limb	

1769

Corresponding Altitudes of the SUN.

Z. D.
° ' "

	'	"	h	'	"	'	"			
June 9	33	43	23	0	0	46	0	Morning	Sun's upper limb	65.32
	46	18	10	40	9	34	0	Afternoon		
	40	16	23	46	34	-	-	Morning	Sun's lower limb	57.0
	39	45	10	33	27	-	-	Afternoon		
	-	-	1	28	43	-	-	Morning	Sun's upper limb	66.44
	-	-	8	51	10	-	-	Afternoon		
	29	12	1	36	9	42	56	Morning	Sun's lower limb	59.26
	50	40	8	43	42	36	54	Afternoon		
10	-	-	23	28	22	34	32	Morning	Sun's upper limb	57.48
	-	-	10	56	56	-	-	Afternoon		
	28	45	23	34	56	-	-	Morning	Sun's lower limb	50.54 hazy
	0	0	11	0	0	-	-	Afternoon		
	-	-	0	59	30	5	50	Morning	Sun's upper limb	64.36
	-	-	9	28	43	22	22	Afternoon		
	59	56	1	6	27	-	-	Morning	Sun's lower limb	55.32
	-	-	9	21	44	-	-	Afternoon		
	-	-	1	20	57	27	32	Morning	Sun's upper limb	51.3
	-	-	9	7	16	-	-	Afternoon		
	21	26	1	28	12	-	-	Morning	Sun's lower limb	50.54
	6	47	9	0	0	-	-	Afternoon		
14	-	-	1	42	4	48	39	Morning	Sun's upper limb	68.20
	-	-	9	27	34	21	1	Afternoon		
	42	31	1	49	18	-	-	Morning	Sun's lower limb	55.32
	27	8	9	20	20	-	-	Afternoon		
	-	-	2	9	8	16	15	Morning	Sun's upper limb	50.54
	-	-	9	0	28	-	-	Afternoon		
	9	38	2	16	58	24	8	Morning	Sun's lower limb	50.54
	0	0	9	0	0	-	-	Afternoon		
	-	-	3	24	23	34	14	Morning	Sun's upper limb	50.54
	-	-	45	8	8	35	15	Afternoon		
	25	6	3	35	12	45	48	Morning	Sun's lower limb	64.36
	44	23	7	34	16	23	38	Afternoon		
18	17	16	0	23	28	29	30	Morning	Sun's upper limb	55.25
	17	13	11	11	3	4	58	Afternoon		
	23	50	0	30	6	36	8	Morning	Sun's lower limb	50.54
	10	40	11	4	2	58	20	Afternoon		
	-	-	0	39	11	45	17	Morning	Sun's upper limb	50.54
	-	-	0	0	0	49	16	Afternoon		
	39	37	0	45	51	-	-	Morning	Sun's lower limb	50.54
	-	-	10	48	43	-	-	Afternoon		
	-	-	2	22	1	29	5	Morning	Sun's upper limb	50.54
	-	-	9	12	22	5	24	Afternoon		
	22	30	2	29	49	-	-	Morning	Sun's lower limb	50.54
	11	54	9	0	0	-	-	Afternoon		
	-	-	3	32	20	41	56	Morning	Sun's upper limb	50.54
	-	-	8	2	7	52	26	Afternoon		
	32	58	3	42	50	53	6	Morning	Sun's lower limb	50.54
	1	27	7	51	35	41	20	Afternoon		

From

From the above corresponding Altitudes the going of the Clock is determined.

	Apparent noon per clock, per equal al- titudes.	Clock too slow for fidereal time.	Rate of clock with respect to fidereal time.
	h ' "	' "	"
May 6	2 53 31,0	1 1,0	+4,6
8	3 1 25,2	0 52,3	+1,2
15	3 28 59,2	0 43,2	-1,3
20	3 48 43,9	0 49,6	
½ June 3	4 45 5,1	1 13,6	-0,5
9	5 9 46,4	1 16,6	+1,3
10	5 13 56,9	1 15,3	+2,1
11	5 18 7,3	1 13,2	-0,2
15	5 34 42,5	1 14,0	+0,07
18	5 47 10,3	1 14,2	
	Mean rate of clock		+0,91

Clock stopped.

TRANSITS taken with a Transit Instrument over the Meridian, on the Island of Magge; or the North Cape of Europe.

1769

½ May 20 | 3^h 49' 30",7 transit of Sun's center at transit instrument, it being adjusted to meridian mark, which was put nearly in the meridian. — And 3^h 48' 43' apparent noon per equal altitudes. By this it appears, the meridian mark is w of the true meridian.

21 | Shifted the meridian mark nearer to the true meridian.

	1 Wire.	2 Wire.	3 Wire.	4 Wire.	5 Wire.	
	' "	' "	h ' "	' "	' "	
½ June 3		43 11½	4 44 1½			⊙ 1 L.
♀ 9			46 17	47 7½		⊙ 2 L.
			5 10 58,6			⊙ 2 L. passed the mer.
	11½	27 8+	18 28 5—	29 2—	58—	α Lyræ.
	59—	43½	19 33 28+	13½	58	α Aquilæ.
½ 10	47 8	47 54	10 48 39	49 24½		⊙ 1 L.
			12 45 14			Polaris S. P.
	32 v	19—	14 4 6—	53½		Arcturus.
	14 v	11—	18 28 7+	4+	0½	α Lyræ.
⊙ 11	15 24—	16 12+	5 17 0½			⊙ 1 L.
			19 18½	20 7½	20 55½	⊙ 2 L.
24 15		32 46½	5 33 34½			⊙ 1 L.
			35 53+	36 41½		⊙ 2 L.
♀ 16	17 52+	18 30—	4 19 26—	20 13+		♀ 2 L.
½ 17	40 18	41 6+	5 41 54+			⊙ 1 L.
			44 15½	45 2½	45 50½	⊙ 2 L.
	33+	3 20½	14 4 7½	4 55		Arcturus.
	42 22+	43 11—	17 43 59+			⊙ 1 L.
			46 17½	47 5½	47 54+	⊙ 2 L.
	26 14	27 11	18 28 7½	4½		α Lyræ.
		17 31—	4 10 17½	19 4½		♀ 2 L.
⊙ 18	44 28—		5 46 24½			⊙ 1 L. } flying clouds.
		47 35—	48 23½	49 12½		⊙ 2 L. }
			12 45 30—			Polaris S. P.

TRANSITS compared with equal Altitudes, for finding the Error of the Meridian Mark.

		Transits of Sun at transit instrument.			Noon per equal altitudes.			Differences.
		h	'	"	h	'	"	
June	3	4	45	9.3	4	45	5.1	4.2 W.
	11	5	18	9.9	5	18	7.3	2.6 W.
	15	5	34	43.9	5	34	42.5	1.4 W.
	18	5	47	13.0	5	47	10.3	2.7 W.
		Mean difference			2.7			Wait.

Hence it appears, that the Sun passed at the transit instrument too late 2".7 by a mean of these observations, which shows the mark was west of the true meridian; which gives the azimuth of mark = 50' south westerly.

TRANSIT OF VENUS, observed at the NORTH CAPE.

1769
June 3 At 13^h 46' 40" per clock, or 9^h 0' 2", apparent time, the Sun came out from under a cloud, with Venus on it, about $\frac{1}{4}$ th of her diameter; and at 14^h 0' 41" or 9^h 14' 1", apparent time, Venus's outer limb seemed to be in contact with the Sun's limb; but no light of that part of the Sun's limb could be seen, Venus being apparently joined to the Sun's limb by a black ligament, which gradually diminished in breadth; and at 14^h 1' 36", or 9^h 14' 56", the Sun's light broke through it, and Venus and the Sun were to appearance perfect (this was certain to about 10 or 15 seconds of time), and the black ligament contracted itself, so that Venus was considerably within the Sun's limb, suppose $\frac{1}{2}$ th of her diameter. During these observations the air was red and hazy, and the Sun's limb very tremulous, and the spots in the Sun very indistinct, and Venus seemed very ill defined when on the Sun. But a better idea will be formed of the bad appearance of Venus at the internal contact, owing to the very hazy state of the air, from the representation of it, plate XIII.

Here follow some measures I was able to take during the time the clouds were off the Sun.

Time per clock.	App. time.	Measures of microm. in inches, &c.	Measures of microm. in degrees, &c.
--------------------	------------	--	---

Soon after the internal contact, measures of Venus's diameter.

0.1 24
0.1 23 +
0.1 23 —
0.1 22 +
0.1 22
0.1 22
0.1 23 —
0.1 24 —
0.1 23 —
0.1 23
0.1 23 —
0.1 23 —
0.1 23 —

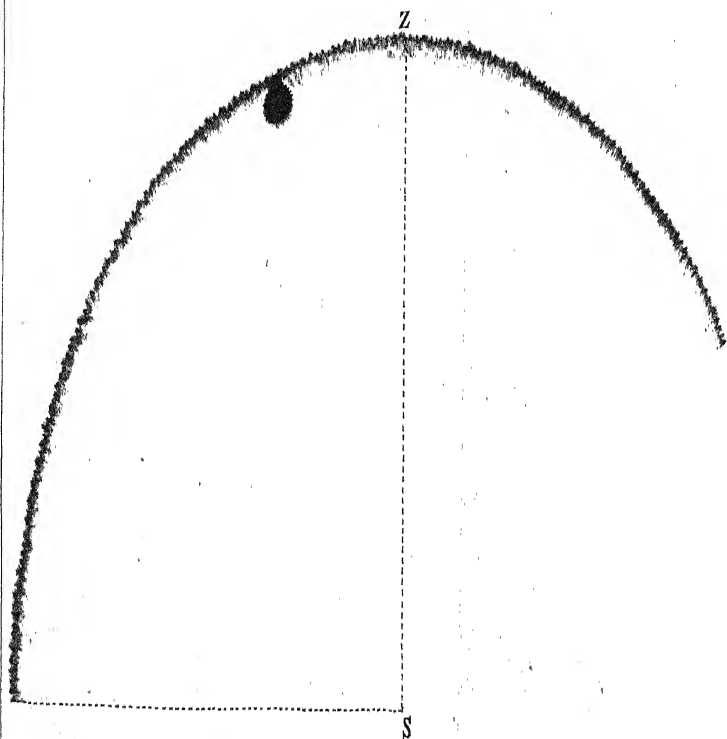
Measures of the Sun's horizontal and vertical diameters, taken immediately after Venus's diameter.

Horizontal Diameter.		Vertical Diameter.	
4.9 $\frac{1}{2}$	21 $\frac{1}{2}$	4.8 $\frac{1}{2}$	12
4.9 $\frac{1}{2}$	23	4.8 $\frac{1}{2}$	10
4.9 $\frac{1}{2}$	23	4.8 $\frac{1}{2}$	11
4.9 $\frac{1}{2}$	21 $\frac{1}{2}$	4.8 $\frac{1}{2}$	9
Mean 4.9 $\frac{1}{2}$	22.2	4.8 $\frac{1}{2}$	10.5

31' 37", 61 | 30' 50", 73

Mean of = 12 | 0.1 22.8 = 55", 32

Difference



Difference of Declinations of the North Limbs
of the Sun and Venus.

Time per clock.			App. time.			Measures of microm. in inches, &c.	Measures of microm. in degrees, &c.
h	'	"	h	'	"	'	"
14	38	37	9	51	50	$0,5\frac{1}{2}$	1 3 29,7
	44	30		57	32	$0,5\frac{1}{2}$	9 3 35,8
	46	56	10	0	8	$0,5\frac{1}{2}$	13 3 38,8
	52	45		5	56	$0,5\frac{1}{2}$	23 3 46,4

Difference of Declination of the South Limbs
of the Sun and Venus.

14	54	50	10	8	1	$4,2\frac{1}{2}$	5 26 58,6
	57	43		10	53	$4,2\frac{1}{2}$	0 26 54,8
	59	52		13	2	$4,2$	20 26 51,0
15	2	0		15	9	$4,2$	16 26 48,0

Equatoreal Distances of the Western Limbs
of the Sun and Venus.

15	8	5	10	21	13	$3,1$	17 19 50,8
	10	28		23	36	$3,1$	2 19 39,4
	12	15		25	23	$3,0\frac{1}{2}$	20 19 34,1
	13	48		26	56	$3,0\frac{1}{2}$	12 19 27,9

Equatoreal Distances of the Eastern Limbs
of Venus and the Sun.

15	15	50	10	28	57	$6,8\frac{1}{2}$	$8\frac{1}{2}$	11 49,4
	17	38		30	45	$1,8\frac{1}{2}$	20	11 58,1
	19	33		32	40	1,9	8	12 8,0
	22	0		35	6	1,9	19	12 16,4
	24	5		37	11	$1,9\frac{1}{2}$	7	12 26,2

Greatest and least Distances of Venus's nearest
Limbs from the Sun's Limbs, for finding
the nearest Distance of their Centers.

15	49	3	11	2	4	$0,6\frac{1}{2}$	17 4 19,7
	51	22		4	23	$4,0$	2+ 25 21,3
	54	24		7	25	$0,7$	7 4 31,3
	58	1		11	1	$3,9\frac{1}{2}$	10 25 8,4
16	0	40		13	39	$3,9\frac{1}{2}$	7 25 6,1
	4	4		17	3	$0,7$	23 4 43,4
	6	24		19	22	$0,7\frac{1}{2}$	5 4 48,8
	9	55		23	53	$3,9$	14 24 52,5
	16	29		29	26	$0,7$	12 4 35,1
	21	31		34	27	$0,7$	8 4 32,1
16	25	55	11	38	51	$3,8\frac{1}{2}$	20::24 38,0::

A Table for reducing the Scale of
the micrometer to Degrees, &c.

Inches.	Value.	1 centis.	Value.	Venus.	Value
'	"	'	"	'	"
1	6 19,95	1	0 37,99	1	0,76
2	12 39,91	2	1 15,99	2	1,51
3	18 59,86	3	1 53,94	3	2,26
4	25 19,81	4	2 31,94	4	3,04
5	31 39,77	5	3 9,97	5	3,80
		6	3 47,97	6	4,56
		7	4 25,97	7	5,32
		8	5 3,96	8	6,08
		9	5 41,96	9	6,84
		10		10	7,60
		11		11	8,36
		12		12	9,12
		13		13	9,88
		14		14	10,64
		15		15	11,40
		16		16	12,16
		17		17	12,92
		18		18	13,68
		19		19	14,44
		20		20	15,20
		21		21	15,96
		22		22	16,72
		23		23	17,48
		24		24	18,24
		25		25	18,99

On examination, the micro-
meter scale wanted no ad-
justment.

ECLIPSE of the SUN, observed at the NORTH CAPE.

1769
June 3 At 1^h 48' 4", the clouds clearing away, I saw the Sun, and the Moon had made a small impression or notch in the Sun's limb; by observing the increase of the eclipse, I suppose it began 4, 5, or 6 seconds sooner than I first saw it, or at 1^h 48' 0" per clock, or 20^h 59' 19" apparent time, nearly.

Distances of the Cusps.

Time p clock	App. time	Measures	Reduced
h ' "	h ' "		' "
2 10 22	21 21 0	3,7 5	23 29,6
13 19	24 34	3,8 $\frac{1}{2}$ 9	24 29,6
19 1	30 15	4,1 $\frac{1}{2}$ 6	26 21,3
21 3	32 16	4,2 7	26 41,1
23 50	35 3	4,3 $\frac{1}{2}$ 8	27 38,8
27 38	38 50	4,4 $\frac{1}{2}$ 14	28 21,4
31 10	41 21	4,5 $\frac{1}{2}$ 10	28 56,4
34 56	46 7	4,6 13	29 17,7
36 4	47 14	4,6 $\frac{1}{2}$ 15	29 38,2
39 11	50 21	4,7 0	29 45,8

Sun's horizontal Diameter, measured directly after the Eclipse ended.

4,9 $\frac{1}{2}$ 21 $\frac{1}{2}$
 4,9 $\frac{1}{2}$ 20
 4,9 $\frac{1}{2}$ 20
 4,9 $\frac{1}{2}$ 22 +
 4,9 $\frac{1}{2}$ 21—

Mean 4,9 $\frac{1}{2}$ 20,9 = 31' 35",9

Measures of the lucid Part, near the Middle of the Eclipse.

2 43 7	21 54 16	1,1 5	7 1,7
44 11	55 20	0,9 $\frac{1}{2}$ 22	6 17,7
47 50	58 59	0,9 24	6 0,2
49 35	22 0 43	0,9 9	5 48,4
52 14	3 22	1,0 $\frac{1}{2}$ 11	6 47,3

Distances of the Cusps.

2 54 55	22 6 2	4,6 $\frac{1}{2}$ 19	29 41,2
57 1	8 8	4,7 3	29 48,1
59 55	11 2	4,6 14	29 18,4
3 24	13 30	4,6 10	29 15,4
4 31	15 37	4,6 7	29 13,1
6 37	17 42	4,5 $\frac{1}{2}$ 9	28 55,6
8 13	19 18	4,5 20	28 45,0
10 34	21 38	4,4 $\frac{1}{2}$ 18	28 24,5
14 57	22 26 1	4,3 17	27 26,7

Clouds came on, so that I saw the Sun no more till 3^h 38' 0" per clock, and it broke away very clear, and continued clear to the end, which was at 3^h 48' 19" per clock, or 22^h 59' 17" apparent time. The air being very clear, the end seemed certain to about two seconds.

The telescope used was a reflector of 2 feet focus, made by Mr. Dollond; and the magnifying power, applied for the ingress of Venus, and the beginning and end of the solar eclipse, was 100. The magnifying power used with the micrometer, was 50.

Adjustments

From the foregoing Zenith Distances of the Sun, and of Arcturus, and α Lyrae, the Latitude of the Observatory is determined, as follows :

		From	Latitude		
			°	'	"
1769					
May	14	☉ U. L.	71	0	43,2
	20	☉ L. L.	71	1	0,1
June	3	☉ Center	71	0	43,5
	11	☉ Center	71	0	39,8
	15	☉ Center	71	0	39,7
	17	☉ Center	71	0	40,6
		Arcturus	71	1	0,9
		α Lyrae	71	0	48,6

Mean - - - - 71 0 47,0

From whence the latitude of the point of land called the North Cape is $71^{\circ} 10'$ north.

By a great many trials with a very good compass, of Dr. Knight's construction, I found the variation to be 6 degrees west ; and by a dipping needle, I found, by repeated trials, the dip of the north end of the needle to be 79 degrees.

May 15, at $13^h \frac{3}{4}$ P. M. apparent time, or $1^h 7'$ after high water, by a mean of 7 observations, I found the dip of the horizon of the sea, from the observatory, to be $12' 18''$. Height of the barometer 29,70 inches ; thermometer, without, 24° ; thermometer, within, 28° . And May 20, at $7^h \frac{3}{4}$ P. M. apparent time, or $7^h 33'$ after high water, from a mean of 8 observations, I found the dip = $12' 25'' ,5$; barometer 29,70 inches ; thermometer, without, 43° , and, within, 40° . Both these observations were made on the N. N. E. point of the true compass. During each of these observations the water was very smooth, and the horizon clear.—I found it was high water, at the full and change of the Moon, at $3^h 44'$ P. M. apparent time, at the Cape ; and, by a series of observations, I found the water to rise 8 feet 1 inch, nearly, perpendicular at the spring tides ; and at neap tides 6 feet 8 inches, perpendicular ; and the tides seemed to follow very regular, as they ought to do when not disturbed by bad weather.—June 8, I found the height of the observatory 140 feet 6 inches above low water mark.

A JOURNAL of the Barometer and two Thermometers, during the Time I was on Shore in the Island of MAGGERCE, or NORTH CAPE, viz. from the 1st of May to the 21st of June.

At noon.

		Bar.	Ther.	
			out	in
1769		Inches	°	°
May	1	29,72	19	24
	2	29,68	26	28
	3	29,90	18	22
	4	29,96	28	$31\frac{1}{2}$
	5	29,92	34	32
	6	29,01	58	38
	7	29,74	60	40
	8	29,84	50	39

		At noon.		At midnight.		
		Bar.	Ther.	Bar.	Ther.	
			out in		out in doors	
		Inches	° °	Inches	° °	
1769						
May	9	29,90	47 36			
	10	29,97	46 39	29,98	34 36	
	11	29,97	39 34	29,96	30 32	
	12	29,97	30 31	29,97	27 29	
	13	30,03	42 36	30,04	34 37	
	14	29,80	43 40	29,74	33 37½	
	15	29,73	41 37	29,70	28 30	
	16	29,66	29 31	29,64	26 28	
	17	29,62	51 42	29,60	39 43	
	18	29,48	53 45	29,39	38 41	
	19	29,34	51 47	29,57	40 40	
	20	29,71	45 41½	29,77	37 41	
	21	29,51	54½ 47	29,46	37 39	
	22	29,51	41 38	29,64	36 43	
	23	29,77	40 37	29,78	36 36½	
	24	29,65	44 41½	29,47	40 40	
	25	29,42	52 45	29,71	44½ 45	
	26	29,77	44 45	29,78	40 42	
	27	29,76	48 44	29,73	36 38	
	28	29,70	44 36½	29,66	34 35	
	29	29,47	38 33	29,51	38 40½	
	30	29,61	50 43	29,61	52 50	
	31	29,61	52 48	29,70	42 37	
June	1	29,67	41 38	29,72	38 40	
	2	29,92	54 50	29,90	48 52	
	3	29,93	66 58	29,78	36 36½	
	4	29,79	43 43	29,70	46 40	
	5	29,77	61 55	29,69	47 49	
	6	29,51	47 43	29,50	43 43	
	7	29,83	43 47	29,90	42 44	
	8	29,89	47 45	29,90	41 40	
	9	29,84	49 47½	29,74	40 42	
	10	29,67	48 42	29,66	41 43	
	11	29,57	46 42	29,58	33 38	
	12	29,48	38 35	29,51	31½ 33	
	13	29,56	38 34	29,52	35 34	
	14	29,53	39 36	29,57	35 35½	
	15	29,66	41 40	29,64	34 35	
	16	29,51	38 38	29,62	38 37½	
	17	29,72	44 42	29,70	52 51½	
	18	29,18	52 54½	29,52	44 46	
	19	29,42	37 39	29,38	36 38	
	20	29,67	36 37	29,49	32 34	
	21	29,54	36 38	29,60	31½ 33	
	22	29,46	35 37			

The thermometers were kept, the one in the observatory, and the other without, in the open air; but always in the shade; and were always observed at noon and midnight, after the 10th of May; but before, only at noon.

The INSTRUMENTS used at the NORTH CAPE, by Mr. BAYLY,
were as follows :

A quadrant of 1 foot radius, and two thermometers, made by Mr. Bird.

A 2 feet reflector, with an achromatic object glass micrometer, by Mr. Dollond.

A transit instrument, of 4 feet, made by Mr. Bird; with an achromatic object glass, by Mr. Dollond.

A barometer, by Mr. Ramsden.

An astronomical clock with a gridiron pendulum, a journeyman clock, and an alarm clock, by Mr. Shelton.

A dipping needle, belonging to the Royal Observatory, made by Mr. Graham.

A like set, exclusive of a dipping needle, was used by Mr. Dixon, at the island of Hammerfoft.

N. B. The adjoining chart, and views of the sea-coast and islands, near the North Cape of Europe, Tab. XIV. were drawn from the joint observations of Messieurs Dixon and Bayly.

XXXVII. *An Account of an Observation of the Transit of Venus, made at Isle Coudre near Quebec. In a Letter to the Reverend Nevil Maskelyne, Astronomer Royal, from Mr. Thomas Wright, Deputy Surveyor of the Northern District of America.*

Quebec, June 15, 1769.

S I R,

Read Nov. 16, 1769. **I** WAS prevented landing at the bay of Gaspée, as I purposed (by blowing, thick weather); but, however, I had the good fortune to reach the island of Coudre, where I landed, with all my apparatus, the 30th of May; and took up my abode at a house well situated, in every respect, for my purpose. The next morning I had a carpenter, who fixed my clock, very firm and perpendicular, against a beam of the house. I immediately set it a-going by my watch, which had not been set to true time for almost a fortnight; but, however, I doubt not but that the following observations of corresponding altitudes will shew exactly the time, as

also the regular rate of going of the clock, which I did not venture to adjust, my time being short.

As it is likely I may stay here some time, and all next winter, I shall endeavour to make such observations as may be useful in further settling the longitude here.

Captain Holland observed the external contact, but not the internal, being prevented by clouds. He has sent them to you by this opportunity.

I am,

SIR,

Your most obedient,

humble servant,

Tho. Wright.

Corresponding double altitudes of the Sun's lower limb, taken with a brass sextant, by reflection, from a saucer of oil, so placed as not to be the least disturbed with wind.

Thursday, June 1, on the north-west side of the island of Coudre, in latitude $47^{\circ} 16' 30''$, determined by several observations of two altitudes, with the interval of time shewn by the time-piece.

Morning, June 1.			Dou. alt.		Afternoon.			Compared separately give					
h	'	"	o	'	h	'	"	h	'	"			
At 8	29	45	75	38	At 4	8	54	12	19	19			
8	32	52	76	42	4	5	37	12	19	14			
8	35	50	77	43	4	2	39	12	19	14			
8	38	31	78	38	4	0	0	12	19	15			
8	40	53	79	26	3	57	24	12	19	08			
8	43	34	80	20	3	55	0	12	19	17			
<hr/>			<hr/>		<hr/>			<hr/>					
8	36	54	Mean		4	1	36	12	19	15	Mean		
16	01	36			Add 12								
<hr/>			<hr/>		<hr/>			<hr/>					
7	24	42	Interval		16	01	36						
<hr/>			<hr/>		<hr/>			<hr/>					
3	42	21	Half Interval										
8	36	54	Time in the morn.										
<hr/>			<hr/>										
12	19	15											
00	00	06	Equat. of corresponding alt.										
<hr/>			<hr/>										
12	19	09	Time shewn by clock at apparent noon										
+2	35		Equation of time—from apparent noon										
<hr/>			<hr/>										
12	21	44	Clock too fast for mean time										
<hr/>			<hr/>										

Friday, June 2.

	Morning.			Dou. alt. ☉ low. limb.	Afternoon.			Compared separately.
	h	'	"		h	'	"	
At	8	54	28	84 19	3	42	20	12 18 24
	8	56	40	85 00	3	40	12	12 18 26
	8	58	50	85 45	3	38	02	12 18 26
	9	2	34	87 00	3	34	13	12 18 24
	9	4	2	87 27	3	32	45	12 18 24
<hr/>					<hr/>			
	8	59	19	Mean	3	37	30	Mean
	15	37	30		12			
<hr/>					<hr/>			
	6	38	11	Interval	15	37	30	
<hr/>					<hr/>			
	3	19	5 $\frac{1}{2}$	Half Interval				
	8	59	19					
<hr/>					<hr/>			
	12	18	24 $\frac{1}{2}$					
			-4	Equat. of corresponding altitudes.				
<hr/>					<hr/>			
	12	18	20 $\frac{1}{2}$	Clock too fast for apparent time				
		+2	26	Equation of time—from apparent				
<hr/>					<hr/>			
	12	20	46 $\frac{1}{2}$	Clock too fast for mean time				
		21	44	Clock too fast at noon of June 2				
<hr/>					<hr/>			
		0	57 $\frac{1}{2}$	Clock has lost in 24 hours				
<hr/>					<hr/>			

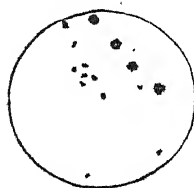
Saturday, June 3, the morning cloudy, no altitudes taken.

h	'	"	
At 2	49	22	by the clock, I happened to take my eye off from the very point where I afterwards found the external contact happened, imagining I saw it something more to westward; but, finding my mistake, I returned to the former point, where I found Venus had made a very small impression at 2 ^h 50' 25", as is set down in the margin.
2	50	25	time when Venus appeared compleatly round to the eye, and to appearance rather detached, and joined by a small dark thread or ligament, which prevented the rays of light from appearing.
3	07	48	time when the rays of light just appeared, at the internal contact.
3	08	19	

The

The following is the above times, as shewn by the clock, reduced to apparent time, by allowing a proportion of .57 seconds, its regular losing in 24 hours; as appears by the preceding and the following corresponding altitudes.

k	'	"	'	"	h	'	"	
2	49	22	—	17	32	=	2	31 50 apparent time of the 1st observation.
2	50	25	—	17	32	=	2	32 53 apparent time of the 2d observation.
3	7	48	—	17	31	=	2	50 17 ap. time of 1st obs. of internal contact.
3	8	19	—	17	31	=	2	50 48 ap. time of 2d obs. of internal contact.



The appearance of Venus at the internal contact, when joined by a small thread to the Sun's limb; as also the spots of the Sun, as observed at the time of the transit, and two days before.

By means of two oblong smoked glasses with different shades, made to slide in a groove fixed to my telescope, the phenomenon appeared very distinct and pleasing to the eye, notwithstanding the weather was a little hazy, and very much so, near the horizon. The thermometer stood at 74 degrees at the time of observation, and the weather was remarkably close and sultry two days before, and quite calm till an hour before the transit happened, when it began to blow very fresh. June 4, the weather continued much the same, and about 9^h 30' in the evening, we had a shock of an earthquake, which lasted about four seconds, and alarmed all the inhabitants of the island.

The weather, at the time of the transit, was not clear enough to observe the least appearance of an atmosphere round the planet, supposing there really had been one.

Saturday,

Saturday, June 3, corresponding double altitudes of the Sun's lower limb for midnight, taken in a faucer of oil.

June 3, Aft. noon.				Altitude.		Morn. June 4.				Compared separately.			
	h	'	''	o	'		h	'	''	h	'	''	
At	4	4	25	76	58		8	29	41	12	17	03	
	4	6	43	76	10		8	27	21	12	17	02	
	4	8	34	75	33		8	25	26	12	17	0	
	4	10	42	74	50		8	23	20	12	17	01	
	4	12	52	74	08		8	21	11	12	17	02	
<hr/>				<hr/>				<hr/>					
	4	8	39	Mean			8	25	24	Mean			
	20	25	24				12						
<hr/>				<hr/>				<hr/>					
	16	16	45	Interval			20	25	24				
<hr/>				<hr/>				<hr/>					
	8	08	22½	Half Interval									
<hr/>				<hr/>				<hr/>					
	4	08	39										
<hr/>				<hr/>				<hr/>					
	12	17	01½	Time of midnight as shewn by clock									
			+9	Equat. of corresponding altitudes									
<hr/>				<hr/>				<hr/>					
	12	17	10½	Clock too fast for apparent time of midnight									
		+2	11	Equation of time—from apparent									
<hr/>				<hr/>				<hr/>					
	12	19	21½	Clock too fast for mean time									
		20	47	Clock too fast, June 2, at noon									
<hr/>				<hr/>				<hr/>					
	1	25½		Clock has lost in 36 hours									
		57½		Clock lost in 24 hours by the preceding observations									
<hr/>				<hr/>				<hr/>					
		28½		Clock lost in 12 hours by the present observation,									
<hr/>				which is very near at the same rate.									

Double altitudes, taken with a sextant, in a faucer of oil, for finding the lat. of the place of observation.

June 4, morn.			Alt. ☉ l. limb.		Afternoon.			Doub. alt. ☉ l. limb.			
	h	'	''	o	'	''	o	'	''		
At	10	34	7	115	12	2	1	50	114	36	} There is 3' to be subtracted from the half ∠ for the er- rors of quadrant.
	10	36	44	116	00	2	4	32	113	46	
	10	37	40	116	13	2	6	24	113	20	
	10	39	26	116	38	2	8	05	112	50	

By

By the first of the above observations with a supposed lat. = $47^{\circ} 15'$, being the result of a former observation, and the Sun's declination (corrected for the longitude) = $22^{\circ} 31' 51''$ N. and half the elapsed $\sphericalangle = 1^h 43' 51'' \frac{1}{2}$ the latitude will be found = $47^{\circ} 16' 51''$, N.

By the second observation, computed in the like manner, the latitude will be $47^{\circ} 16' 41''$, N.

The place of observation on the island of Coudre, by an actual survey, bears from Quebec, N. $41^{\circ} 30'$, E. by the true meridian, distance 55 statute miles, = 52 marine; which gives D. latitude = $39'$ and Dep. $34' = 50'$ D. longitude = $3' 20''$ of time between Quebec and Coudre.

I have here mentioned every particular relative to the observation, and as it really happened, that you might, with greater certainty, correct any errors that may be found therein.

To prove the time ascertained by corresponding equal altitudes, those altitudes taken within an hour of the transit might be worked separately, remembering to subtract $3'$ from the single altitude for the error of the quadrant.

Remarks by the ASTRONOMER ROYAL.

THE instruments made use of by Mr. Wright, in the foregoing observations, were a 2 feet reflecting telescope; a pendulum clock beating half seconds; a brass Hadley's sextant, of about 15 inches radius, with a magnifying glass to read off the observations; and a rectangular reservoir for holding quick-silver,

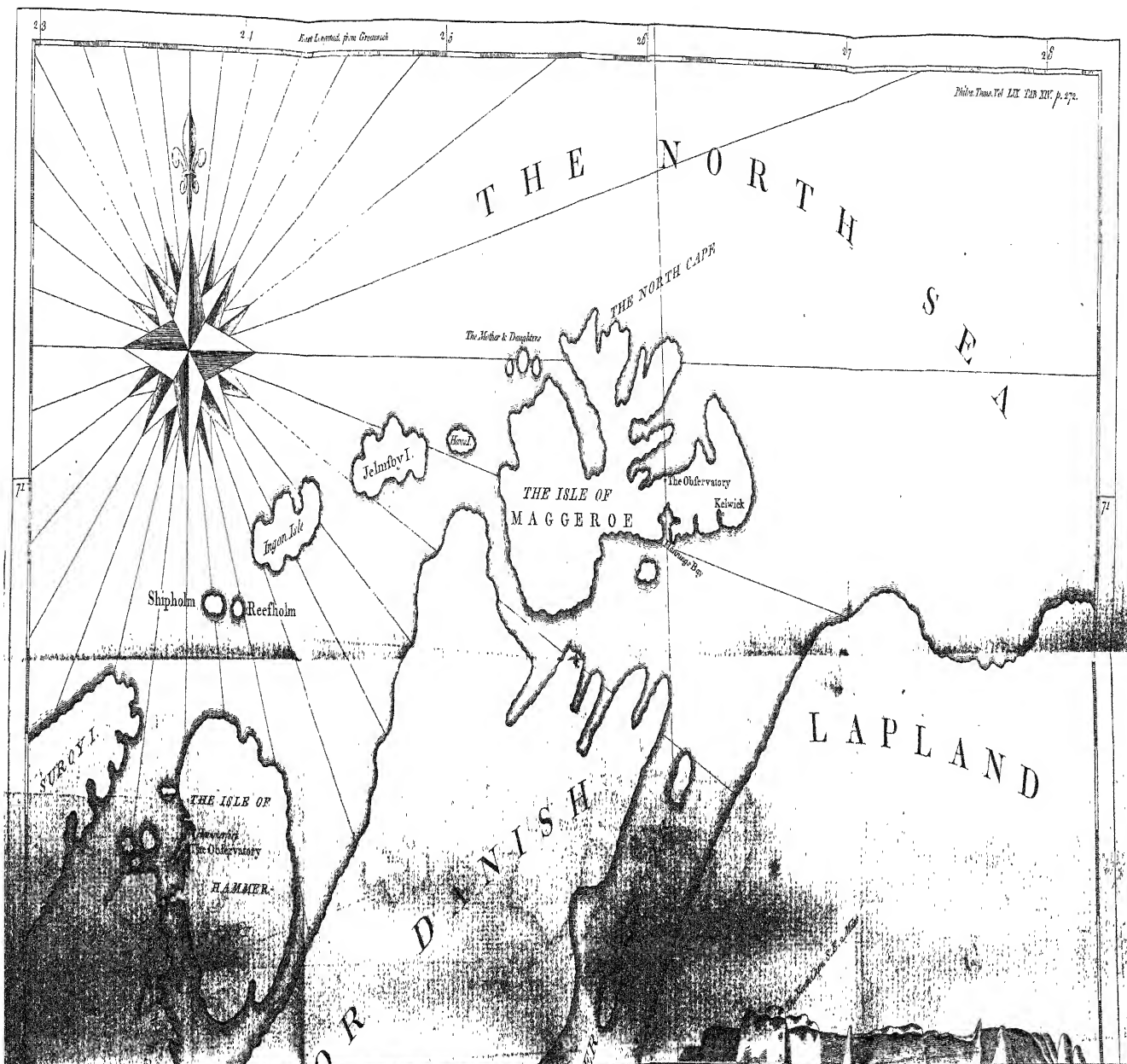
silver, or any other fluid, which is sheltered from the wind by two glass sides inclined to one another, and ground truly plane: this last for taking the Sun's double altitude by reflection with the Hadley's sextant. By a more accurate calculation of the times than Mr. Wright has used, I find the equation of corresponding altitudes, for the noon of June 1 to be $-5''$, 0, June 2 $-4''$, 5, and June 3 for midnight $-9''$, 6. Hence the true time of noon, by the clock, June 1, was $12^h 19' 10''$, 0; June 2, $12^h 18' 20''$, 0; and June 3, midnight, $12^h 17' 11''$, 1; and hence the true time of noon, June 3, should be $12^h 17' 34''$, 1, and the clock is losing $46''$ per day on apparent time. Hence the apparent times of Mr. Wright's 4 observations will come out as follows:

App. time.

$h \quad ' \quad ''$

- $2 \quad 31 \quad 53$ No visible impression made by Venus yet.
- $2 \quad 32 \quad 56$ Venus had made a small impression.
- $2 \quad 50 \quad 19$ Venus appeared completely round to the eye, and rather detached, and joined by a ligament.
- $2 \quad 50 \quad 50$ The rays of light appeared at the internal contact.

Taking Isle Coudre to bear N. $41^\circ 30'$ East from Quebec, distant 55 statute miles, as, Mr. Wright says, was found by an actual survey; the distance in geographical miles is 47,65. Therefore the place of observation is $35' 41''$ north of Quebec, and $31' 34''$ east of it, $= 46' 32''$ difference of longitude, $= 3 \quad 6''$ of time.



XXXVIII. *Extract of a Letter from Mr. B. Gooch, Surgeon, of Shottisham, near Norwich, to Mr. Joseph Warner, F. R. S. and Surgeon to Guy's Hospital. Communicated to the Royal Society by Mr. Warner, November 16, 1769.*

Shottisham, September 9, 1769.

DEAR SIR,

Read Nov. 16,
1769.

ACCORDING to your desire, and my promise, I have sent you the wonderful cuticular glove, which I shewed you, when I had the pleasure of your company here. The history of the case, which, I believe, has no precedent, is taken from the gentleman's own relation of it to me in writing, without varying his sense; and confirmed by Mr. Swallow, a surgeon of character at Watton, whose son, I know, was under your tuition. Mr. Swallow attended the patient many times in the fevers which produced these strange phenomena, with whom I took an opportunity of having a particular conversation relative to this matter, that I might be able to speak with the more authenticity. Mr. Swallow has now one of these gloves in his possession; the gentleman himself has another, and several he has given to the curious: yet some have been so sceptical

as to doubt the matter of fact upon such evidence and authority. I wish you would get an accurate drawing of the glove; and I shall be glad to know, at your leisure, the sentiments of the learned, not forgetting your own, upon this extraordinary case.

I am, DEAR SIR,

Your sincere friend,

as well as obliged,

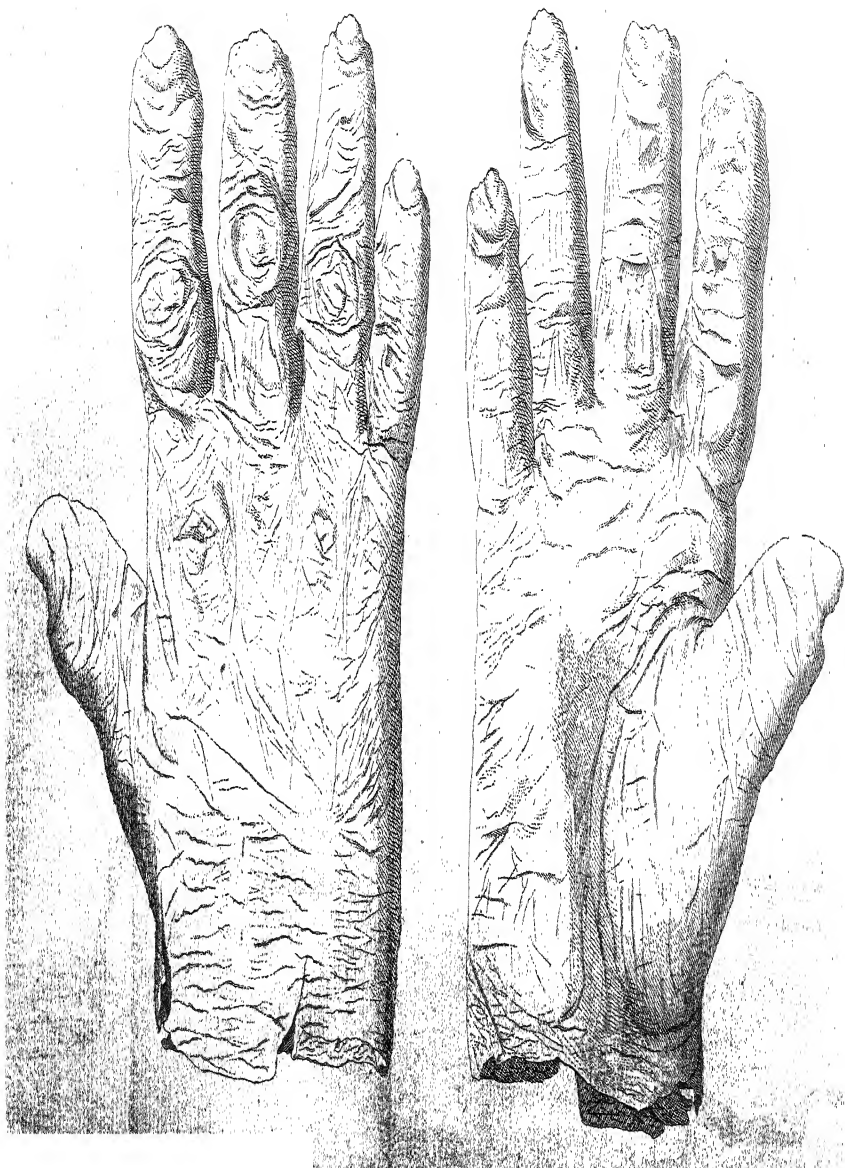
humble servant,

R. Gooch.

History of the CASE relating to the CUTICULAR GLOVE.

M^{R.} WILLIAM WRIGHT, of Saham Tony in the county of Norfolk, attorney at law, about fifty years of age, rather of a weak and lax constitution from his youth, was first seized about ten years ago with the following singular kind of fever. The physical gentlemen he at different times consulted were at a loss to know what name or character to distinguish it by. It has returned many times since; sometimes twice in a year, attended with the same symptoms and circumstances; but not to so great a degree since the year 1764 as before; and it has been generally observed to come on upon obstructed perspiration, in consequence of catching cold, to which he is very subject.

Besides



Besides the common febrile symptoms upon the invasion of this disease, his skin itches universally, more especially at the joints; and the itching is followed by many little red spots, with a small degree of swelling: soon after his fingers become very stiff, hard, and painful at their ends, and at the roots of his nails. In 24 hours, or thereabouts, the cuticle begins to separate from the *cutis*, and, in ten or twelve days, this separation is general from head to foot; when he has many times turned the cuticle off from the wrists to the fingers ends, completely like gloves; and in the same manner also to the ends of his toes; after which his nails shoot gradually from their roots, at first attended with exquisite pain, which abates as the separation of the cuticle advances; and the nails are generally thrown off by new ones in about six months.

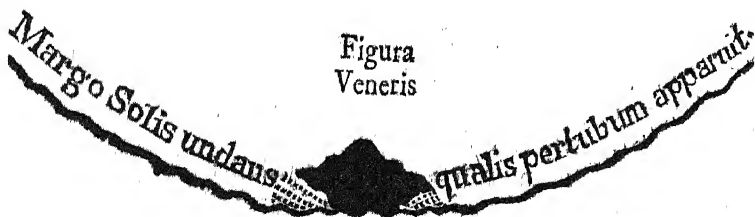
The cuticle rises in the palms of his hands, and soles of his feet, resembling blisters, but has no fluid under it; and when it comes off, it leaves the subjacent skin very sensible for a few days. Sometimes, upon catching cold, before he has been quite free from feverish symptoms, he has had a second separation of the cuticle from the *cutis*, but then it is so thin as to appear only like scurf, which demonstrates the quick renewal of this part.

* * The figure of one of these gloves is seen in
*
TAB. XIV.

XXXIX. *Observatio ingressus Veneris in
Solem 3 die Junii, 1769, habita Gryphis-
waldiæ, ab Andr. Mayer, Prof. Reg.
Communicated by Mr. John Ellicot,
F. R. S.*

Read Nov. 16, 1769. **P**ER omnem diem cælum ita nubibus tectum erat, ut fere nulla hujus rarissimi phænomeni spectandi spes superesset, donec circa horam septimam vespertinam in ea cœli parte, quam Sol occupaverat, nubes primum rarefcere cœperunt; atque post breve temporis spatium liberum Solis conspectum concesserunt. Non tamen sudum omnino aërem, sed vaporibus adhuc repletum fuisse, undabundus Solis margo satis superque docebat. Usus sum tubo Dollondiano, egregiæ virtutis, septem pedes longo, quem postquam in eam marginis Solaris partem direxeram, a Venere in ingressu suo juxta calculum hujus rei gratia institutum contingendam, octava hora 4' 35'' temporis veri, eodem fere temporis momento, quo etiam Prof. et observat. Reg. clariss. Dn. Réhl per tub. refr. 16½ ped. Solem intuenti Venus ingredi visa erat, apparuit mihi unda in margine Solis nonnihil major atque nigricantior reliquis, quam discum Veneris fuisse paulo post convictus reddebar. Accuratiorem hujus ingressus observationem negabat omnino athmosphæræ nostræ conditio.

ditio. Inter continuas et magnas undarum reciprocaiones Venus ulterius progressa, difformem valde et mutabilem semper præbebat faciem: ultra dimidium ingressæ diameter horizontalis tribus fere vicibus superabat verticalem, lumenque Solare intra margines Veneris et Solis contentum magis flavesceus reliquo imo subrufilum mihi videbatur. Ipsa Veneris facies, distorta nimium, irregularis, atque continuo obnoxia flexui, per reliquum observationis tempus talis erat, qualem figura adjecta exhibet. Paulo ante contactum interiorem, fascia quasi margini Solis alligata visa est, quæ subito soluta $8^h 22' 44''$ temp. ver. docebat ingressum fuisse factum, cujus mora secundum hanc observationem, erat $18' 9''$. Nulla nec satellitis Veneris, nec ejus atmosphæræ vestigia, quæ 1761 nobis tam distincte apparuere, nisi huc referre liceat, undæ incitatioem motum in ingressu disci Veneris, atque luminis intra margines Solis et Veneris a reliquo discrepantiam. Nullus in tanta titubatione phænomeni micrometri usus erat, aut filorum, hunc in finem tubis applicatorum. Observatio quidem in suburbii tugurio habita est, ab observatorio ad 2000 passus remoto. Non solum ad penduli motum ex observationibus alt. Solis corresp. correctum tempora notata sunt, sed et beneficio soni ex sclopetis emissi reducta ad motum excellentissimi penduli Ellicotiani in observatorio suspensi.



Puncta designant loca, in quibus
lumen Solis subrufilum videbatur.

Read November 16, 1769.

XL. *Observation of a Solar Eclipse the 4th of June, 1769, at the Observatory at Aufthorpe, near Leeds, in the County of York. By J. Smeaton, F. R. S.*

	h	'	"
BEGINNING by mean time, A. M.	6	33	I
Middle - - - - -	7	26	38
End - - - - -	8	20	16
Total duration - - - - -	1	47	15
Digits eclipsed - - - - -	6	46	

N. B. The beginning and end of the eclipse were observed by an excellent $3\frac{1}{2}$ feet treble object-glass telescope, constructed by Dollond, with the smallest magnifier, which enlarged the diameter somewhat above 80 times. As there is no defect in quantity of light from the Sun, the object glass was contracted by an aperture to $2\frac{1}{2}$ inches, and the object was perfectly sharp and distinct.

The quantity was taken by a parallel wire micrometer, upon an equatoreal apparatus, which rendered it very commodious for the purpose; by which the part of the Sun's diameter, remaining uneclipsed, measured at right angles to a line joining the horns, was 889 such parts as the Sun's diameter, taken the same

same day at $1\frac{1}{2}$ in the afternoon, measured between two parallels of declination, 2041 .

The latitude I have not yet got so correctly as I expect to do; but I do not at present know, whether it exceeds or falls short of $53^{\circ}48'$. The supposed longitude is $6'$ of time west of Greenwich; this is deduced from its position with Wakefield, whose longitude is set down in Maskelyne's British Mariner's Guide, as determined from an observation of the transit of Venus, 1761.

The exact knowledge at what point of the Sun's circumference to look for the beginning (which was communicated to me by Mr. Maskelyne), I found of great use; insomuch that, I believe, I saw the first discernible impresson; I have, however, allowed $2''$ for the time elapsed between the first perception, and the being sure it was the approach of the Moon that affected that part of the Sun's limb; and which latter only could be noted by the clock. The first approach did not, however, affect the Sun's circumference by any thing like a penumbra or shade; but began by some asperities of the Moon's limb, seeming to thrust themselves into that of the Sun; and which appeared before any continued part of the Sun's circumference was cut off; or, perhaps, it might be occasioned by the first approach of the Moon's limb, disturbing the little protuberances upon the Sun's circumference, occasioned by the undulation of the air, and which, when rendered exceedingly distinct, appeared almost like the teeth of a fine saw. This whole appearance, to a telescope less distinct, would probably look like a penumbra or shadow.

Some time before the great spot was immersed, there appeared two parts of the Moon's circumference more protuberant than the rest, near the right hand horn; which so remarkably interrupted the regularity of the curve, that it was taken notice of by all about me; and which, doubtless, was occasioned by two mountains upon the Moon's surface, remarkably higher than the rest; and I doubt not but the same thing will have occurred to other observers.

* * Mr. Smeaton was prevented, by clouds, from
 * observing the entrance of Venus upon the Sun,
 the evening before.

END OF PART I.

PHILOSOPHICAL TRANSACTIONS.

VOLUME LIX. PART II.

PHILOSOPHICAL
TRANSACTIONS,
GIVING SOME
ACCOUNT

OF THE
Present Undertakings, Studies, *and* Labours,

OF THE
INGENIOUS,

IN MANY
Considerable Parts of the WORLD.

VOL. LIX. PART II. For the Year 1769.

L O N D O N :

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near *Gray's-Inn Gate*, in *Holbourn*.

M.DCC.LXX.

PHILOSOPHICAL TRANSACTIONS.

PART II.

XLI. *Account of the Transit of Venus over the Sun's Disk, as observed at Norriton, in the County of Philadelphia, and Province of Pennsylvania, June 3, 1769. By William Smith, D. D. Provost of the College of Philadelphia; John Lukens, Esquire, Surveyor-General of Pennsylvania; David Rittenhouse, A. M. of Norriton; and John Sellers, Esquire, one of the Representatives in Assembly for Chester County; the Committee appointed for that Observation, by the American Philosophical Society, held at Philadelphia, for promoting useful Knowledge. Communicated to the said Society, in Behalf, and by Direction, of the Committee, by Doctor Smith; and to the Royal Society of London, by Nevil Maskeline, B. D. Astronomer Royal.*

GENTLEMEN,

Read Nov. 23,
1769.

AMONG the various public-spirited designs, that have engaged the attention of this Society since its first institution, none

does them more honour than their early resolution to appoint Committees, from their own members, to make as many observations, in different places, of that rare phenomenon, the transit of Venus over the Sun's disk, as they had any probability of being able to defray the expence of, either from their own funds, or the public assistance they expected.

As the members of the Committee above mentioned live at some distance from each other, I am therefore now, by their direction, and in their behalf, to collect, and lay before you, the whole of the Norriton observations; distinguishing however, so far as may be necessary, the part of each observer; and going back to the first preparations. For I am persuaded that the dependance which the learned world will place on any particular transit account, will be altogether in proportion to the previous and subsequent care, which they find hath been taken, in a series of accurate and well-conducted observations, for regulating the time-pieces, and ascertaining the latitude and longitude of the place of observation, &c. And I am the more desirous to be particular in these points, in order to do justice to Mr. Rittenhouse, one of our Committee; to whose extraordinary skill and diligence is owing whatever advantage may be derived in these respects to our observation of the transit itself. It is further conceived, that the learned and curious will be desirous to have not only the work relative to each particular transit account, but the materials also, that they may have an opportunity to examine and conclude for themselves. And it is a pleasure to us that we are able to communicate so complete a set of observations in every material article.

The great discouragement which the different Committees laboured under, at their first appointment by this Society, was the want of proper apparatus, especially good telescopes. The generosity of our Provincial Assembly soon removed a very considerable part of this discouragement, not only by their vote to purchase one of the best reflecting telescopes, with Dollond's micrometer, but likewise by their subsequent benefaction of one hundred pounds, for erecting observatories, and defraying other incidental expences. It was foreseen that, on the arrival of this telescope*, added to other private ones that could be procured in this city, and the fitting up the instruments belonging to the honourable Proprietaries, viz. the large astronomical sector, and the equal altitude and transit instrument, nothing would be wanting for the city observatory, erected in the State-house Square, but a good time-piece, which was easily to be procured.

We remained, however, still at a loss how to furnish the Norriton observatory. But this difficulty gradually vanished. Early in September, 1768, soon after the nomination of the Committees, I received a letter from that worthy and honourable gentleman, Thomas Penn, Esquire, one of the Proprietors of this province, expressing his desire that we would exert ourselves in observing the transit, for which our situation was so favourable; and inclosing some copies of Mr. Maskelyne's directions for that purpose.

* This telescope came to hand in April, 1769. It was made by Nairne, $4\frac{1}{2}$ feet, and its greatest magnifying power 400 times; with an excellent micrometer.

This gave an opportunity, which I embraced immediately, to inform Mr. Penn, that this Society, before the receipt of his letter, had appointed two* Committees for the business he proposed; that the Assembly had generously ordered one hundred pounds, sterling, to purchase one telescope, as above set forth; but that we should be at a great loss for a telescope and micrometer, of the like construction, for the Norriton observatory; requesting him to order a reflector of $2\frac{1}{2}$ feet, with Dollond's micrometer, to be got ready in London; which I was in hopes I should prevail on our College to pay for, and take for their own use, as soon as the corporation should have an opportunity of meeting. It was not long before I had the pleasure to hear that Mr. Penn had ordered such a telescope, which came to hand in due time, with a most obliging letter, expressing his satisfaction in the spirit shewn at Philadelphia for observing this curious phenomenon, and concluding as follows:

“ I have sent by Captain Sparks a reflecting telescope, with Dollond's micrometer, exact to your request, which I hope will come safe to hand. After making your observation with it, I desire you will present it, in my name, to the College. Messieurs Maſon and Dixon tell me, they never used a better than that which I formerly sent to the Library Company of Philadelphia, with which a good observation† may be made, although it has no micrometer.”——

* A third Committee was afterwards appointed, to observe at or near Cape Henlopen.

† Mr. Owen Biddle, who was appointed by the Society to conduct the observation near Cape Henlopen, had this telescope;

We were now enabled to furnish the Norriton observatory, as follows :

1. A $2\frac{1}{2}$ feet Gregorian reflector, with a Dollond's micrometer, made by Nairne, its magnifying powers 55, 95, 135, and 200 times. The gift of the honourable Thomas Penn, Esquire, to the College of Philadelphia. Used by Doctor Smith.
2. A refractor of 42 feet, its magnifying power about 140. The glasses sent to the Assembly, with the large reflector, from England. Used by Mr. Lukens.
3. Mr. Rittenhouse's refractor, with an object glass of 36 feet focus, and a convex eye glass of 3 inches, magnifying about 144 times. Used by himself.
4. An excellent clock ; a transit telescope, nicely moving in the plane of the meridian ; and a very accurate equal altitude instrument, supported, in the observatory, on a stone pedestal. These three articles were also Mr. Rittenhouse's private property, and made by himself, whereof some mention is made below.
5. An astronomical quadrant, $2\frac{1}{2}$ feet radius, made by Sisson ; the property of the proprietors of East New Jersey ; under the care of the right honourable William Earl of Stirling, surveyor-general of that province, who kindly permitted us the use of it.

nothing being desired here but the contacts, and their exact time, which he obtained to great satisfaction, as by his report may appear.

As

As Mr. Rittenhouse's dwelling at Norriton is 20 miles N. W. of Philadelphia, our other engagements did not permit Mr. Lukens, or myself, to pay much attention to the necessary preparations; but we knew that we had entrusted them to a gentleman on the spot, who had, joined to a complete skill in mechanics, so extensive an astronomical and mathematical knowledge, that the construction, use, and management of all the necessary apparatus are perfectly familiar to him. The dull and rainy weather prevented our setting out for his house till Thursday, June 1; and we found, on our arrival there, every preparation so forward, that we had little to do but to examine and adjust our respective telescopes to distinct vision. Mr. Rittenhouse had compleated his observatory, fitted up the different instruments, and made a great number of observations, for fixing the latitude and longitude of the observatory, and ascertaining the going of his time-piece. The laudable pains he has taken in these material articles will best appear from the work itself, which I received from him a few days ago, with the following very obliging letter; giving me a liberty which his own accuracy, care, and abilities leave me no room to exercise; and therefore what follows is entirely as he drew it up, viz.

" To the Reverend Doctor SMITH.

" Dear Sir,

" The enclosed is the best account I can give of
 " the contacts, as I observed them, and of what I
 " saw during the interval between them. I should
 " be glad you would contract them (and the other
 " papers)

“ papers) into a smaller compass, as I would have
 “ done myself, if I had known how. I beg you
 “ would not copy any thing merely because I have
 “ written it, but leave out what you think super-
 “ fluous.

“ I am, with great esteem and affection, Yours,

Norriton, July 18, 1769.

“ DAVID RITTENHOUSE.”

Mr. Rittenhouse's account of the observations made by him, at Norriton, before and after the transit of Venus, June 3, 1769; for determining the longitude and latitude of his observatory, regulating his clock, &c.

“ Early in November, 1768, I began to erect an
 “ observatory, agreeable to the resolutions of the
 “ American Philosophical Society; but, through
 “ various disappointments from workmen and wea-
 “ ther, could not complete it till the middle of
 “ April, 1769. I had for some time expected the
 “ use of an equal altitude instrument from Philadel-
 “ phia; but finding I could not depend on having
 “ it, I fell to work and made one, of as simple a
 “ construction as I could contrive. It has a good
 “ telescope of $3\frac{1}{2}$ feet focal length, with two hori-
 “ zontal and one vertical wire, in the focus, and is
 “ very easily adjusted to a plummet wire, 4 feet in
 “ length, by two screws; one of which moves it in
 “ a north and south, the other in an east and west
 “ direction. The 20th of March this instrument

" was finished, and put up out of doors, the observatory being not yet ready.

" I had, however, for some weeks before this, with my 36 feet refractor, observed eclipses of Jupiter's first satellite, in such a manner, that though my equal altitude instrument was not finished, and consequently I could not set my time-piece to apparent noon, I should, notwithstanding, be able to tell the true time of those eclipses afterwards, when the instrument should be finished. For this purpose, I observed, almost every fair evening, the time by the clock, when the bright star in Orion disappeared behind a fixed obstacle, by applying my eye to a small sight-hole, made through a piece of brass, fastened to a strong post. The observations were as follows:—

1769.	Star disappeared per clock.	Immersions of 1st satellite, per clock.	Equal altitudes observed.	Hence the Sun on meridian per clock.	
Day.	h m s	February, Day. h m s	March, Day. h m s	h m s	
Febr. 15	9 26 39	16 14 24 58	19 { 8 58 52	2 56 52	11 57 37
22	8 58 52	23 16 17 41	9 { 2 13	2 53 32	
24	8 50 57				
March 3	8 23 21	Hence from the equal altitudes in column 3; the apparent times of the two above immersions are	20 { 8 56 40	2 58 26	11 57 18
12	7 48 26		8 { 59 59	2 55 7	
14	7 40 41				
17	7 29 4				
20	7 17 16	Feb. h m s			
21	7 13 21	16 14 21 10			
28	6 45 44	23 16 15 1			

" From March 20 to May 20, the clock was altered several times, once taken down to be cleaned, removed back to the observatory, and regulated anew. Care, however, was taken to observe equal altitudes

“ altitudes of the Sun on the day preceding and following any visible eclipse of the 1st satellite.
 “ The eclipses and other observations, during this period, were the following:—

Equal altitudes of the Sun.			Sun on meridian per clock.	Observed immersions of 1st satellite.	Equal altitudes of the Sun.			Sun on meridian per clock.	Observed eclipses of 1st satellite.
April 3, 1769.				April 3.	May 4th.				
A. M.		P. M.		Immersion	A. M.		P. M.		
h ' "	h ' "	h ' "	h ' "	h ' "	h ' "	h ' "	h ' "	h ' "	h ' "
8 5 22	4 1 56		12 3 25	14 52 40	8 5 15	3 44 6			
8 8 16	3 59 2				8 8 3	3 41 18	11 54 32		
					8 9 23	3 39 48			
4th.					5th.				May 5th.
8 3 43	4 3 3		12 3 9		8 4 11	3 44 51			Immersion
8 6 38	4 0 10				8 6 59	3 42 4	11 54 22		11 23 45
					8 8 19	3 40 42			
10th.				10th.	6th.				
8 32 8	} cloudy.			Day-light	8 3 8	3 45 37			
8 35 6				16 46 20	8 5 54	3 42 51	11 54 14		
8 36 31					8 7 15				
11th.					11th.				
8 30 22	3 36 43		12 0 20		8 34 51	3 17 12			
8 35 6	3 27 47				8 36 13	3 15 49			
8 36 31	3 26 22				8 37 40	3 14 22	11 55 54		
12th.				12th.	8 39 3	3 12 59			
8 28 55	} cloudy.			11 14 38	15th.				May 14th.
8 31 51					9 12 59	2 39 28			Emerison
8 33 16						2 38 2	11 56 7		9 58 20
14th.					9 15 53	2 36 32			
8 25 42	3 33 56					2 35 7			
Cloud	3 31 1		11 59 38						
8 30 2	2 29 37								

“ May 20, in the morning, the clock was set up
 “ for the last time, pretty near the mean time. It
 “ had no provision for preventing the irregularities
 Vol. LIX. Q q arising

“ arising from heat and cold ; nor could I find lei-
 “ sure to apply any contrivance of this sort. It had
 “ been made some time before, to determine the va-
 “ riations arising from those causes. The pendulum-
 “ rod is a flat steel bar, with a bob weighing about
 “ twelve pounds, and vibrates in a small arch. It
 “ goes eight days, beats dead seconds, does not
 “ stop when wound up, and is kept in motion by a
 “ weight of five pounds.

“ The ill state of my health would not permit
 “ me to sit up at nights, to take equal altitudes
 “ of the stars. I was therefore obliged to content
 “ myself with those of the Sun only. I had,
 “ some time before this, viz. May 12, got a te-
 “ lescope fixed in the meridian, on an axis with
 “ fine steel points ; so that the hair in its focus
 “ could move in no other direction than along
 “ the meridian. I set up two marks, north and
 “ south, about 60 rods distant each, to which it
 “ can readily be adjusted, in a horizontal position,
 “ by a screw ; as it can likewise by another screw in
 “ a vertical position. The two marks were shifted
 “ from day to day, until they were found within less
 “ than one second of time of the true meridian.

“ May 20, I likewise put wires, instead of hairs,
 “ in the telescope of the equal altitude instrument ;
 “ and the following are the observations taken both
 “ with it, and with the meridian or transit telescope,
 “ in the order wherein they were made :

	Equal altitudes of the Sun.	Hence the Sun on meridian.	Observed eclipses of Jupiter's satellites.	Observations with the meridian telescope.	Hence the Sun's center on meridian.
	A. M. h' ' "	P. M. h' ' "			
May	O's up. limb } at 1st hair } 8 1 30 3 51 28 D° at 2d hair } 8 2 52 3 50 8 O's l. limb } at 1st hair } 8 4 15 3 48 45 D° at 2d hair } 8 5 36 3 47 24				
20			Emerfions.	O's western limb at meridian } 11 55 16 O's eastern limb at ditto } 11 57 31	
21	N. B. As the Sun seems to descend, being inverted in the telescope, his up. limb is set down as coming to the upper hair first—though they might as properly be called 1st and 2d as upper and lower hairs	8 1 1 3 52 11 Clouds { 3 50 50 { 3 49 27 { 3 48 7	1st Satellite. Em. 11 51 46	O's western limb 11 55 23 O's eastern ditto. 11 57 37 ♀'s center on merid. 1 18 39	
23		8 0 4 3 53 36 8 1 24 3 52 16 8 2 47 3 50 53 8 4 8	11 56 45	O's western limb 11 55 39 Eastern ditto 11 57 53	11 56 46
24				O's eastern limb 11 58 0 Passage of O's semi-diameter } 0 1 8 ♀'s center on meridian hair } 1 2 4	11 56 52
25	7 59 15 3 54 57 8 0 35 3 53 38 8 1 58 3 52 15 8 3 18 3 50 54	11 57 1		O's western limb 11 55 53 Eastern ditto 11 58 9	11 57 1
26	7 58 54 3 55 38 7 58 18 3 54 18 8 1 37 3 52 56 8 2 57 3 51 35			O's western limb 11 56 3 Eastern ditto 11 58 18	11 57 10 1/2

June	Observed equal Altitudes of the Sun.				Hence the Sun's in meridian.	Observed eclipses of Jupiter's satellites	Observations with the meridian telescope.	Hence the Sun's center on me- ridian.
	A M		P.M.		h ' "	h ' "	h ' "	h ' "
8	☉'s up. limb at 1st hair	7 56 27	4 3 12					
	Ditto at 2d hair	7 57 48	4 1 52				☉'s west limb on merid.	11 58 40
	☉'s L. limb at 1st hair	7 59 12	4 0 28	11 59 48			E. limb ditto	12 0 57
	Ditto at 2d hair	8 0 32	3 59 7					
10		7 56 22	4 4 1					
		7 57 48	4 2 41		12 0 9 $\frac{1}{2}$			
		7 59 12	4 1 17					
		8 0 32	3 59 7					
12							☉'s west limb East ditto	11 59 29 12 1 45
13		7 59 13	4 2 30		12 0 50	1st satellite. Em. 12 5 59	☉'s west limb East ditto	11 59 42 12 1 59
		8 0 33	4 1 11					12 0 50 $\frac{1}{2}$
14							☉'s west limb East ditto	11 59 57 12 2 13
16		7 56 52	4 6 16				☉'s west limb East ditto	12 0 26 12 2 42
		7 58 12	4 4 57	12 1 34			☉'s center on merid.	12 1 34 9 6 4
		7 59 36	4 3 33					
		8 0 56	4 2 12					
17							☉'s west limb + Passage of se- mi-diameter	12 0 36 1 08.8
							☉'s center on meridian	12 1 44 $\frac{1}{2}$ Thermo- meter at } 77
								9 1 50
19							☉'s west limb + Passage of se- mi-diameter	12 0 56 1 08.8
							☉'s center on meridian	12 2 4 $\frac{1}{2}$ Therm. 77° 8 53 24
21							☉'s west limb East ditto	12 1 17 12 3 34
								12 2 25 $\frac{1}{2}$ Therm. 83° Observed

June	Observed equal altitudes of the Sun.		Hence the Sun on merid. an.	Observed eclipses of Jupiter's satellites.	Observations with the meridian telescope.		Hence the Sun's center on me- ridian.
	A. M. h ' "	P. M. h ' "			h ' "	h ' "	
22					☉'s west limb East ditto	12 1 28 12 3 45	12 2 36½ Therm. 74½
23					☉'s west limb East ditto	12 1 39 12 3 55	12 2 47 Therm. 73½
24					☉'s west limb East ditto	12 1 49 12 4 5	12 2 57 Therm. 84°
25				3d satellite out of the shadow, on applying the eye at 8 ^h 54' 39".	☉'s west limb East ditto	12 1 57 12 4 14	12 3 5½ Therm. 80°
26					☉'s west limb East ditto J's east limb on merid.	12 2 6 12 4 23 18 13 52	12 3 14½ Therm. 85°
27					☉'s west limb East ditto J's east limb on merid. U's center on merid.	12 2 14 12 4 31 19 4 19 8 19 58	12 3 22½ Therm. 88°
28	☉'s up. limb at 1st hair Ditto at 2d hair ☉'s l. limb at 1st hair Ditto at 2d hair	7 59 11 8 0 31 8 1 55 8 3 15	4 7 45 4 6 25 4 5 0		☉'s west limb East ditto	12 2 21 12 4 38	12 3 29½
29	8 0 48 8 2 11 8 3 32	4 7 43 4 6 22 4 4 59 4 3 38		1st satellite ap- peared from be- hind a cloud; at 10 ^h 25' 1".	☉'s west limb East ditto	12 2 29 12 4 45	12 3 37 June 30, Therm. 85°

	Observed equal altitudes of the Sun.	Hence the Sun's on meridian.	Observed eclipses of Jupiter's satellites	Observations with the meridian telescope.	Hence the Sun's center on me- ridian.
	A. M. P. M. h ' " h ' "		3d satellite. h ' "	h ' "	h ' "
July					
2	☉'s up. limb } at 1st hair } Ditto at 2d hair } ☉'s l. limb at } 1st hair } Ditto at 2d hair }	8 0 24 4 7 28 8 1 44 4 6 8 8 3 8 4 4 4 8 4 29 4 3 23	Im. 11 19 36	☉'s west limb East ditto	12 2 52 12 5 8 12 4 0 Therm. 81° $\frac{1}{2}$
3		8 0 46 4 7 21 8 2 7 4 6 52 8 3 31 4 4 37 8 4 51 4 3 16		☉'s west limb East ditto	12 2 58 12 5 15 12 4 6 $\frac{1}{2}$ Therm. 83°
4		8 1 9 4 7 13 8 2 30 4 5 32 8 3 53 4 4 28 8 5 14 4 3 8		☉'s west limb East ditto	12 3 6 12 5 23 12 4 14 $\frac{1}{2}$ Therm. 87°
5		8 5 36 4 2 57		☉'s west limb + Passage of se- mi-diameter }	12 3 11 1 8,5 12 4 19 $\frac{1}{2}$ Therm. at 3 o'clock 94° $\frac{1}{2}$
8		8 1 36 4 7 41 8 2 56 4 6 20 8 4 19 4 4 57		☉'s western limb Eastern ditto	12 3 36 12 5 52 12 4 44 Therm. 85° $\frac{1}{2}$

Table of Eclipses of Jupiter's Satellites, observed at Norriton ; compared with the calculated times of the same Eclipses for Greenwich, in order to fix the longitude of the Norriton observatory. N. B. If the observed times of such of those Eclipses as have been seen at Greenwich should differ from the calculated times, the following difference of longitude, thence deduced, must be corrected accordingly.

1st satellite. Immersions at Norriton.				Calculated apparent time of the				Long. of Norriton,			
apparent time.				same for Greenwich.				west of Greenwich,			
								thence deduced.			
1769. Day.	h	'	"	Day.	h	'	"	h	'	"	
Feb. 16	14	21	10	Feb. 16	19	22	29	5	1	19	
23	16	15	1	23	21	16	35	5	1	34	
April 3	14	49	25	April 3	19	51	24	5	1	59	
10	16	46	0	10	21	47	14	5	1	14	
12	11	14	39	12	16	16	13	5	1	34	
May 5	11	29	27	May 5	16	31	20	5	1	53	
Emersions.				Emersions.							
21	11	55	13	21	16	56	49	5	1	36	
June 6	10	11	32	June 6	15	12	59	5	1	27	
13	12	5	1	13	17	6	31	5	1	30	

Difference of longitude from a mean of the above 9 eclipses										5	1 34

3d satellite, at Norriton; apparent				Calculated time				Hence diff.			
time.				for Greenwich.				of long.			
Day.	h	'	"	h	'	"		h	'	"	
June 25	8	51	33	Emersion	13	58	34	5	7	1	}
July 2	11	15	36	Immersion	16	12	29	4	56	53	
Mean diff. of longitude								5	1	57	

N. B. As the emerision happened so much sooner, and the immersion later, than the time given by the Tables, it is concluded, that the satellite did not dip so deep in the shadow as the Tables would have it; but the mean of both gives nearly the same difference of longitude as the first satellite.

“ May

" May 20, Mr. Lukens sent up the astronomical
 " quadrant, belonging to the East-Jersey proprietors,
 " of $2\frac{1}{2}$ feet radius ; which I placed in the meridian,
 " and observed the following zenith distances of stars,
 " to discover the error of the instrument, if any it had.

Observations with the face westwards.					Ditto with the face eastwards.				
		°	'	"			°	'	"
Highest star in the leg of Bootes.	May 31	20	36	6	Same star.	June 6	20	35	55
	June 4	20	36	0		7	20	35	54
	5	20	36	0		8	20	36	0
Arcturus.	May 31	19	46	18	Arcturus.	June 6	19	46	5
	June 1	19	46	14		7	19	46	8
	2	19	46	20		8	19	46	13
	5	19	46	22		10	19	46	11

Zenith dist. with face of quadrant westwards.

Zen. dist. face of quad. east.

The bright star in the Crown.	June 1	12	39	36	June 6	12	39	34
	5	12	39	27		10	12	39 18

" From a mean of the above 18 observations, the
 " error of the quadrant is $3''\frac{1}{2}$ to be subtracted from
 " the zenith distance, when the face is westwards, and
 " added when it is eastward.

Zenith distances of the Sun's limbs observed, and the latitude of the
 observatory deduced separately from each.

Sun's up. limb; dist. to zenith.					Lat. hence deduced.					Sun's l. limb; dist. to zenith.					Lat. hence deduced.				
		9	1	"		0	1	"			0	1	"		0	1	"		
May	25	18	48	45		40	10	17	June	8	17	29	33		40	9	48		
	26	18	38	18		40	10	10		9	17	24	35		40	9	47		
	27	18	28	21		40	10	10		10	17	20	5		40	9	49		
June	1	17	43	47	cloudy and doubtful					11	17	15	59		40	9	52		
	2	17	36	16		40	10	2		14	17	6	9		40	9	58		
Doubtful	4	17	21	51		40	9	52											
	6	17	8	53		40	9	34											
	7	17	3	21		40	9	47											
	12	16	41	10		40	10	14											
	13	16	37	45		40	10	8											
Mean of the above observations of ☉'s upper limb					40 10 1					Mean of the above obser- vations of ☉'s lower limb					40 9 51				
Mean of the 5 obser. of ☉'s l. limb					40 9 51														

The mean of both, viz. 40 9 56 is taken for the latitude of the observatory.

“ The difference of the above observations is
 “ greater than might be wished. All that I can of-
 “ fer to excuse them is the want of better instru-
 “ ments; although I think the differences were
 “ much owing to the action of the Sun on the
 “ wooden frame, which supported the quadrant.
 “ For I always found, that when the shutter in the
 “ roof was opened, the plummet-wire would, in a
 “ minute or two, leave the point, although it had
 “ stood quietly over it all the forenoon. Never-
 “ theless, a *mean* from so many observations may be
 “ supposed very near the truth; since, if we leave
 “ out that of June 6, which differs most from the
 “ others, the *mean* of them will be but 2'' greater
 “ than the latitude set down above.”——

So far I have given Mr. Rittenhouse's account of his observations, previous and subsequent to the transit; for regulating his time-piece, and fixing the latitude and longitude; containing many months work, *viz.* from February 15 to July 8. More observations have been taken since, but the above are thought sufficient.

It hath been already mentioned, that it was not till Thursday afternoon, June 1, that Mr. Lukens and myself arrived at Norriton, with a design to continue with Mr. Rittenhouse till the transit should be over. When we set out, the prospect before us was very discouraging. That day, and several preceding, had been constantly overcast with clouds, and frequent heavy rains, a thing not common for so long a period at that season of the year, in this part of America. But, on Thursday evening, by one of those sudden transitions which we often experience here, the
 weather

weather became perfectly clear in less than the space of one hour, and continued the day following, as well as the day of the transit, in such a state of serenity, splendor of sunshine, and purity of atmosphere, that not the least appearance of cloud was to be seen in the whole heavens.

June 2, and the forenoon of June 3, were spent in making the necessary preparations; such as, examining and marking the *foci* of the telescopes, particularly the reflector with and without the micrometer, and in its different powers. The reflector was also placed on a polar axis; and such supports were contrived for resting the ends of the refractors, as might give them a motion as nearly parallel to the plane of the equator as such hasty preparations would permit. Several diameters of the Sun were taken during this time, and the micrometer examined by such other methods as the time would allow.

The Sun was so intensely bright on the day of the transit, that it was found best, early in the forenoon, to lay aside the coloured glasses, brought with the reflecting telescope from England; and to put on deeply-smoked glasses, which Mr. Lukens prepared, in their room; and which gave a much more beautiful and well-defined appearance of the Sun.

Mr. Rittenhouse, on a supposition of the Sun's horizontal parallax being but $8''$, had, in one of his calculations of the time of the transit, made in September, 1768, brought out the first external contact, at Philadelphia, to be June 3, $2^h 11'$, mean time. It happened, that he was not many seconds wrong in this, although most other calculations made it from $6'$ to $8'$ later for Philadelphia.

We thought it prudent therefore, at one o'clock, to take off the micrometer from the reflector, which had been used with a magnifying power of 95 times; and, after adjusting the *focus*, continued the same power for the reasons mentioned * below, in order to observe the transit; and lest the external contact might happen still sooner than the earliest predicted time, it was resolved, during the hour from one till two, to keep an alternate watch, through the reflector, on that half of the Sun's limb where the contact must happen; while those not thus employed were making all other preparations as follows, *viz.*

1. That each of us might the better exercise our own judgment, without being influenced, or thrown into any agitation or surprize by the others; it was agreed to transact every thing by signals, and that one observer should not know what the others were doing. The situation of the telescopes, the reflector being within the observatory, and the two refractors, mounted at some distance from each other without it, favoured this purpose. Wherefore,

2. Two persons, *viz.* Mr. Sellers, one of our committee, for whom no telescope could be pro-

* As the refracting telescopes gave but a small field, and were very unmanageable, on account of their length, and the Sun's great altitude, it was thought best to use a smaller power and larger field with the reflector, that if the contact should happen at a different part of limb than where it was expected, one of us, at least, might be sure not to miss it, but give notice to the others. It was agreed, however, that if the contact happened at or very near the part of the limb where we did expect it, no such notice was to be given. It was, although, thought best to have some difference in the magnifying powers; and the vision with the reflector was so distinct and well defined, that I am well pleased I used no higher power.

vided,

vided, and Mr. Archibald M'Cleau, both men of abilities, and accustomed to astronomical observations, were placed in one window of the observatory, to count the clock, and take the signals from Mr. Lukens. Two others, who live in Mr. Rittenhouse's family, and have been trained by him to services of this kind, stood in another window, within the observatory, to count the time, and take his signals. I was within hearing of the beats of the clock, and was to count and set down my own time.

These preliminaries thus settled, at two o'clock, each of us applied to our respective telescopes; but as there was a great concourse of many of the principal inhabitants of the county, we were apprehensive, that our scheme for silence and order might be interrupted by the impatience and curiosity natural on such occasions. We therefore informed the gentlemen, who had honoured us with their company, that the accuracy and success of our observations would depend on our not being disturbed with the least noise, till the contacts were over. And to do the company justice, during the 12 minutes that ensued before the first contact, there could not have been a more solemn pause of expectation and silence, if each individual had stood ready to receive the sentence that was to give him life or death. So regular and quiet was the whole, that, far from hearing a word spoken, I did not even hear the feet of the four counters, who had passed behind me from the windows to the clock; and I was surprized, when I rose up and turned to the clock, to find them all there before me, counting up their seconds to an even number; as I imagined,

from

from the deep silence, that my associates had yet seen nothing of Venus.—

As the contacts are reckoned to be one of the most essential articles relative to this phenomenon, it is material, before we set down their times, to give a particular account of the manner in which each observer judged of them, and the other circumstances attending them.

MR. RITTENHOUSE'S ACCOUNT OF THE CONTACTS.

“ At 2^h 11' 39" per clock, the Reverend Mr.
 “ Barton, of Lancaster, who assisted me at the te-
 “ lescope, on receiving my signal, as had been
 “ agreed, instantaneously communicated it, by wav-
 “ ing a handkerchief, to the counters at the window,
 “ who, walking softly to the clock, counting as they
 “ went along, noted down their times separately,
 “ agreeing to the same second. And three seconds
 “ sooner than this, to the best of my judgment, was
 “ the time when the least impressiion made by Venus
 “ on the Sun's limb could be seen through my te-
 “ lescope.

“ When the Planet had advanced about one third
 “ of her diameter on the Sun, as I was steadily
 “ viewing its progress, my sight was suddenly at-
 “ tracted by a beam of light, which broke through
 “ on that side of Venus yet off the Sun. Its figure
 “ was that of a broad-based pyramid; situated at
 “ about 40 or 45 degrees on the limb of Venus,
 “ from a line passing through her center and the
 “ Sun's

“ Sun’s, and to the left hand of that line, as seen
 “ through my telescope, which inverted. See TAB.
 “ XV. fig. 1.—About the same time, the Sun’s light
 “ began to spread round Venus on each side, from the
 “ points where their limbs intersected each other, as is
 “ likewise represented in fig. 1.

“ As Venus advanced, the point of the pyramid
 “ still grew lower, and its circular base wider, until
 “ it met the light which crept round from the points
 “ of intersection of the two limbs: so that when
 “ half the Planet appeared on the Sun, the other
 “ half was entirely surrounded by a semicircular
 “ light, best defined on the side next to the body of
 “ Venus, which continually grew brighter, till the
 “ time of the internal contact. See fig. 2.

“ Imagination cannot form any thing more beau-
 “ tifully serene and quiet than was the air during the
 “ whole time; nor did I ever see the Sun’s limb
 “ more perfectly defined, or more free from any
 “ tremulous motion; to which his great altitude
 “ undoubtedly contributed much. When the in-
 “ ternal contact (as it is called) drew nigh, I fore-
 “ saw that it would be very difficult to fix the time
 “ with any certainty, on account of the great breadth
 “ and brightness of the light which surrounded that
 “ part of Venus yet off the Sun. After some con-
 “ sideration, I resolved to judge as well as I could of
 “ the co-incidence of the limbs; and accordingly
 “ gave the signal for the internal contact at $2^h 28' 45''$
 “ by the clock, and immediately began to count
 “ seconds, which any one, accustomed to it, may
 “ do, for a minute or two, very near the truth. In
 “ this manner, I counted no less than $1' 32''$ before
 “ the

“ the effects of the atmosphere of Venus on the
 “ Sun’s limb wholly disappeared, leaving that part
 “ of the limb as well defined as the rest. From
 “ this I concluded, that I had given the signal too
 “ soon ; and the times given by the other observers
 “ confirm me in this opinion.”

Mr. LUKENS’s Account of the CONTACTS.

“ The telescope I used, being a refracting one of
 “ 42 feet, giving but a small field, and something
 “ difficult to manage ; I was obliged to move often,
 “ and apprehend that I did not discover the first
 “ external contact exactly. For, after one of those
 “ movements, on bringing the glass to bear again on
 “ that part of the Sun’s limb where Venus was ex-
 “ pected, I saw a large tremulous shadow, already
 “ somewhat advanced, and seeming to press still in-
 “ wards on the Sun’s limb. Having contemplated
 “ this for a few seconds, and perceiving the appear-
 “ ance grow more dark, and make a better-defined
 “ impression on the limb, I gave the signal to the
 “ persons who counted time for me, which they
 “ noted down separately at $2^h 12' 3''$ by the clock.
 “ When Venus was near one half her diameter
 “ advanced on the Sun, I saw a border of light en-
 “ compassing that part of her which was yet off the
 “ Sun. This was so bright, that it rendered that
 “ part of Venus visible, and pretty well defined, al-
 “ though off the Sun. But, towards the internal
 “ contact,

“ contact, the circular border of light became of a
 “ more dusky colour, especially at the two points
 “ where the luminous segments of the Sun’s limb
 “ were ready to close round the Planet. This
 “ duskiness did not seem to part wholly from the
 “ Sun’s limb, at the time I apprehended the body of
 “ Venus to be wholly entered on the Sun, and when
 “ I gave the signal for the internal contact; which
 “ was noted at $2^h 28' 58''$ by the clock. And I
 “ judge from $6''$ to $8''$ more, before I saw the Sun’s
 “ limb clear of this dusky surrounding shadow, and
 “ as well defined as before the first contact.”

Dr. SMITH’S Account of the CONTACTS.

“ Having, for reasons already assigned, determined
 “ to continue one of the smaller powers of the Grego-
 “ rian reflector, for observing the contacts (viz. that
 “ which we had been using, and were again to use, with
 “ the micrometer, magnifying 95 times), I had a large
 “ field, taking in, at least, one half of the Sun’s disk; and
 “ the telescope was so firmly supported (with its axis
 “ in a polar direction), that it could not be shaken
 “ by any motion on the earthen floor of the observa-
 “ tory, and required only a small movement of one
 “ of the handles of the rack-work to manage it.
 “ With these advantages, it was easy to keep any
 “ part of the Sun’s limb in the middle of the field,
 “ without neglecting to cast my eye, every three or
 “ four seconds, on every other part of the limb

VOL. LIX. S s “ on

“ on both sides, where there was any possibility of
“ the contact to happen,

“ It being now within about half a minute of the time
“ which Mr. Rittenhouse had calculated for the first
“ contact, I called to the four counters at the win-
“ dows, to be very attentive to those who were to give
“ the signals from the telescopes without doors ; and
“ turning my eye to that part of the limb where the
“ contact was expected, I had been, for several se-
“ conds, viewing it stedfastly, when, all at once, I saw
“ something strike into it like a watry pointed sha-
“ dow, appearing to give a tremulous motion to that
“ part of the Sun’s limb, although the telescope
“ stood quite firm, and not the least disturbance or
“ undulation was perceptible in any other part of
“ the limb.

“ The idea I had formed of the contact was—
“ that Venus would instantaneously make a well-
“ defined black and small dent or impressiion on the
“ Sun. But this appearance was so different, the
“ disturbance on the Sun’s limb so undulatory,
“ pointed, ill-defined, waterish, and occupying a
“ larger portion of the limb than I expected, that I
“ was held in a suspense of five or six seconds, to
“ examine whether it might not be some small skirt
“ of a watery flying cloud. But perceiving this
“ shadow, or whatever else it was, to press still for-
“ ward on the limb, with the same tremulous,
“ pointed appearance (the longest points towards its
“ middle), I began to count the clock for either
“ fifteen or sixteen seconds, when a well defined
“ black dent, occupying a less space on the Sun’s
“ limb, became plainly visible. I then quitted the
“ telescope,

“ telescope, and turned to the clock, to note this
 “ time for the contact, which was $2^h 12' 5''$. About
 “ $22''$ sooner than this (being doubtful to two or
 “ three seconds at first) was the first impression I
 “ saw on the limb; which I have marked accord-
 “ ingly at $2^h 11' 40''$ to $43''$. If this first impres-
 “ sion is to be taken for the external contact, I think
 “ it might be judged of to a single second of time;
 “ which one could not do by several seconds, either
 “ with respect to the internal contact, or even with
 “ respect to the moment of the distinct black dent
 “ made at the external contact, both which are far
 “ from being instantaneously perceptible. Whether
 “ a telescope of larger powers than what I used
 “ might not have shewn this first impression sooner
 “ (be it an atmosphere or whatever else), I will not
 “ determine; though from Mr. Rittenhouse's time
 “ I think it probably would. But I am sure that I
 “ saw the first stroke that was perceptible through
 “ my telescope, and might have noted it to a single
 “ second, had I expected it in that way.

“ As to the internal contact, the thread of light,
 “ coming round from both sides of the Sun's limb,
 “ did not close instantaneously, but with an uncer-
 “ tainty of several seconds, the points of the threads
 “ darting into each other, and parting again, in a
 “ quivering manner, several times before they finally
 “ adhered. I waited for this adherence with all the
 “ attention in my power, and noted it down for the
 “ internal contact at $2^h 29' 5''$, a few seconds later
 “ than Mr. Lukens, who took the same method of
 “ judging.

“ Having quitted my telescope to note down the
 “ time, the gentlemen who counted for us, as well
 “ as several others now come into the observatory,
 “ were anxious to see Venus on the Sun through the
 “ reflector, as it was easily manageable; an indul-
 “ gence not to be denied; and therefore I did not sit
 “ down to it again till about four or five minutes
 “ before the internal contact, and then not with
 “ much attention till the contact was at hand; so
 “ that I saw none of those appearances, on the part
 “ of Venus off the Sun, mentioned by my associates.
 “ But their account may be depended on; for Mr.
 “ Rittenhouse’s abilities have been spoken of before;
 “ and few persons have a better judgment, a correcter
 “ eye, or have been more accustomed to view ob-
 “ jects, both celestial and terrestrial, through te-
 “ lescoptes, than Mr. Lukens.

“ As to the small differences in the times of our
 “ contacts, it is presumed, they may be easily re-
 “ conciled, partly from the different powers of the
 “ telescopes, and partly from the other circumstances
 “ mentioned in the manner of judging of them.
 “ At any rate, we have set them down faithfully.

“ As to the first disturbance made on the Sun’s
 “ limb, it may be worth considering, whether it was
 “ really from the interposition of the limb of Venus,
 “ or of her atmosphere? The former, one could not
 “ easily imagine it to be, unless her limb and body
 “ were much more uneven than they appeared to
 “ be when seen on the Sun. An atmosphere it
 “ might more probably seem to be, not only from
 “ the faintness of the colour, but the undulatory
 “ motion,

“ motion, which might arise from the growing den-
 “ sity of the atmosphere, as it pushes forward on the
 “ Sun, varying the refraction of the rays. If such an
 “ atmosphere be allowed, then it probably gives the
 “ same tremulous motion, at the internal contact, to
 “ the thread of light creeping round Venus; and
 “ prevents its closing quietly till the atmosphere (or
 “ at least its densest part) be wholly on the Sun; and
 “ consequently the true coincidence of the limbs be
 “ past. For though the atmosphere of Venus can-
 “ not be seen on the Sun, yet that part which is fur-
 “ rounding, or just entering on the Sun’s limb, hav-
 “ ing, as it were, a darker ground behind it, may be
 “ visible. But these are only little conjectures sub-
 “ mitted to others; though if they have any founda-
 “ tion, it would make some difference in the time
 “ estimated between the contacts.”

General Table of the contacts of the limbs of the Sun and Venus, as observed at Norriton, June 3, 1769, reduced to apparent time.

N. B. June 3, by the preceding Tables of the work, the Sun's center was on the meridian, at $11^h 58' 49''$ by the clock, and June 4, at $11^h 59' 2''$, and therefore gained $13''$ in 24 hours of apparent time. Wherefore at noon June 3, the clock being $1' 11''$ slow of apparent time, it was only $1' 10''$ slow at the observation of the contacts. Whence

The apparent time of the different contacts was :

External contact, by Dr. Smith.	Extern. contact, by Mr. Lukens.	External contact, by Mr. Rittenhouse.
$h \quad m \quad s \quad ''$ A visible impression on the Sun's limb, in form of a quivering dusky shadow, with many points } $2 \ 12 \ 58 \text{ to } 53''$ Uncertain to } or $4''$. A well defined black dent in the Sun's limb, at } $2 \ 13 \ 15$	$h \quad m \quad s \quad ''$ A small dent in the Sun's limb } $2 \ 13 \ 13$	Judged of as } $h \quad m \quad s \quad ''$ described in his } $2 \ 12 \ 49$ account
Internal contact.	Internal contact.	Internal contact.
Judged from a thread or crescent of light, closing round the dark body of Venus, with a tremulous motion, at } $2 \ 30 \ 15$	$2 \ 30 \ 8 \text{ to } 14$	Judged of as } $2 \ 29 \ 55$ described in fig. 3d of his account, Plate xv.

" When Venus was fully entered on the Sun's
 " limb, and we had satisfied ourselves by comparing
 " our different notes of the contacts, which were
 " thrown together on the table of the observatory, we
 " prepared for the micrometer, and other observa-
 " tions. The greatest part of the micrometer obser-
 " vations were taken by me, while Mr. Rittenhouse
 " undertook to take another set of observations;
 " namely,

“ namely, the appulses of the limbs of the Sun, and
 “ the center of Venus, to the cross hairs of the equal
 “ altitude telescope ; Mr. Lukens taking and writing
 “ down the time for him.

The whole observations, reduced to apparent time, are as follows :

No. of observations.	App. time. June 3, 1769.			Micrometer measures of the least distance of the nearest limbs of the Sun and Venus.		Angular value of the micrometer measures.		Parallax of Venus from the Sun, adjusted to the times of the micrometer observations, for projecting the transit; by Mr. Rittenhouse.		
	h	m	s	Inches	seconds	'	"	In the vertical	In the path of Venus.	Perpendicular to the path.
								"	"	"
R. 1	3	7	19.	0	4. 0,5	1	45,4	14,54	13,67	4,94.
R. 2	3	11	39.	0	4 12	1	57,6	14,74	13,88	4,96
L. 3	3	17	42	0	5 2	2	13,5	15,09	14,24	5,01
R. 4	3	32	3.	0	6 14	2	52,7	15,77	14,92	5,13.
R. 5	3	40	4.	0	7 4	3	8,6	16,17	15,32	5,23
6	4	35	5	0	10 21,5	4	46,67	18,45	17,45	6,5
7	4	57	9	0	11 19,5	5	11,05	19,02	17,95	6,32.
8	5	7	49.	0	11 22,75	5	14,5	19,5	18,36	6,63
9	5	21	40.	0	11 23,5	5	15,3	19,88	18,64	6,98.
10	5	31	46	0	11 21,5	5	13,7	20,12	18,8	7,23
11	5	42	38	0	11 17,5	5	8,93	20,36	18,95	7,48
12	5	51	10	0	11 13,5	5	4,7	20,52	19,06	7,67
13	6	22	4	0	10 5,5	4	29,7	21,0	19,21	8,57
14	6	31	5	0	9 20	4	18,58	21,12	19,22	8,82.
15	6	41	24.	0	9 0	3	57,38	21,22	19,15	9,14
L. 16	6	48	12	0	8 13	3	44,66	21,26	19,12	9,31
17	6	53	30	0	8 1,5	3	32,47	21,28	19,04	9,49
R. 18	6	56	22	0	7 23	3	28,76	21,29	19,02	9,56

Distance of the limbs of the Sun and Venus, taken in chords, parallel to the plane of the equator.							Parallax of Venus from the Sun, adjusted as above.		
R.	1	3 58 53	☉'s e. limb	0 17 14,3	7 44,57	17,0	16,1	5,40	
	2	4 27 18	Ditto	1 3 6	10 14,74	18,16	17,18	5,86	
	3	6 4 27	Ditto	2 0 20	18 0,08	20,75	19,16	8,06.	
	4	6 9 28	☉'s w. limb	0 15 6,5	6 43,27	20,81	19,2	8,2	

Observations.	No of Ob- s.	Diameters of Venus on the Sun, June 3, 1769.			Diameters of the Sun, June 2, P. M.		
		P. M.	Inches	seconds	h	inch.	seconds
R.	1	3 0	0 2	4.5	3	50	40
	2	3 2	0 2	4.5	3	52	0
L.	3	3 4	0 2	5	3	11	15
	4	4 15	0 2	4.7	3	11	14
	5	5 55	0 2	4.7	3	11	14.5
R.	6	5 58	0 2	5—			

A. M. June 3.			
R.	8 35	0	3 11 13.5
L.	8 40	0	3 11 16—
	8 45	0	3 11 13.5
P. M.			
	12 35	0	3 11 13
R.	12 40	0	3 11 12.5
	4 40	0	3 11 10.5

The above times are set down by the clock, according to the vulgar reckoning; as are all the micrometer observations of the Sun's diameters.

From a mean of the above six diameters of Venus on the Sun, allowing for the error of adjustment, as mentioned below :

Venus's diameter, for the day of the transit,	0 57.3
The Sun's semidiameter, from a mean of the five horizontal diameters, taken the same day	15 47.0
Or, from a mean of four, taken that day, leaving out the second, which Mr. Lukens thinks he may have taken too large	15 45.0

All the micrometer observations were separately reduced to their value in minutes and seconds, both by Mr. Rittenhouse and myself. Many more might have been taken; but as so many persons were desirous of looking through the telescope, they could not well be denied; and the number above set down are found fully sufficient for all the purposes of the projection; especially as they have been found to agree so well with each other.

Such of the micrometer measures as were taken by Mr. Rittenhouse or Mr. Lukens, are marked with the initials of their names. All the others I am answerable for.

Our observations being thus finished, Mr. Rittenhouse was pleased to undertake the projection of the transit

transit from them; and his account of the work follows :

Delineation of the transit of Venus over the Sun, according to the Norriton observations, with the principles of the work. By Mr. Rittenhouse. See Tab. XV. fig. 4.

“ The Sun’s horizontal parallax is assumed $8''.65$
 “ at his mean distance from the earth; from which,
 “ and the observed least distance of the centers of the
 “ Sun and Venus, the chord for the transit line was
 “ laid down. The semi-diameters of the Sun and
 “ Venus are taken as by the above observations.
 “ One point in the transit-line was then fixed by the
 “ first micrometer distance of the limbs at $3^h 7' 19''$
 “ apparent time. This line was carefully divided in-
 “ to hours and minutes, on the supposition that Ve-
 “ nus moves $240'',36$ over the Sun’s disk in an hour,
 “ according to a calculation I had formerly made
 “ from Halley’s Tables. The place of Venus’s cen-
 “ ter in the transit-line was then marked to the times
 “ of each of the micrometer observations, and from
 “ thence the *apparent place* of her center found, by
 “ setting off the quantity of her parallax from the
 “ Sun in its proper direction. About each of the
 “ centers so found, a circle is described with the rad.
 “ $28'',6$, her observed semi-diameter. Blank lines
 “ were next drawn through the Sun’s center, and the
 “ apparent place of the center of Venus. On these
 “ the red lines were drawn from the Sun’s limb, pre-
 “ cisely of such length as we found them by the mi-
 “ crometer; so that it may be seen at once how
 “ far the micrometer measures agree with each
 Vol. LIX. T t other,

“ other, by observing how much they exceed or fall
 “ short of reaching the limb of Venus. Out of the
 “ 18 that were taken, 14 of them correspond so well
 “ that I am convinced they may be depended upon.
 “ The 4th which I have set down, and only one
 “ other, which will be found omitted, differ some-
 “ thing from what they ought to be; which might
 “ easily happen, either from any mistake in noting
 “ the time, or in reading the *vernier* of the micro-
 “ meter, or not fixing it exactly in the direction of
 “ the nearest distance of the limbs; tho’ great care was
 “ taken in this part, by sweeping it constantly round
 “ to try the truth of the contacts that were formed.

“ The measures intended to be taken in chords
 “ parallel to the equator, are very near the truth, con-
 “ sidering that, in setting the micrometer to that di-
 “ rection, we had only the truth of the polar axis to
 “ depend on, which was constructed hastily to an-
 “ swer the purpose of the day, and was not exactly
 “ true, as a small motion of the rack-work that raises
 “ or depresses the telescope was sometimes necessary
 “ to keep the Sun in the field. Three of these mea-
 “ sures, parallel to the equator, agree with each
 “ other, and with all the rest of the micrometer ob-
 “ servations, on supposing the chord in which they
 “ were taken inclined half a degree to the plane of
 “ the equator. The fourth of these measures is still
 “ more nearly parallel to the equator, but diverges a
 “ little the other way. These chords are delineated
 “ in the * projection, and serve to confirm the other
 “ work.

* It was intended also to have confirmed the projection still
 further, by the observations made of the appulses of the limbs of

“ All

“ All the parallaxes of Venus from the Sun were
 “ taken from a large projection on a scale of half an
 “ inch to one second, and then reduced to the scale
 “ of this delineation. After calculating some of
 “ those parallaxes, and finding those given by the
 “ projection constantly true to the first decimal place,
 “ any further nicety was thought needless.

0 ' "

The angle of Venus's visible way }
 with the ecliptic, I find } 8 28 27

The angle of the ecliptic with a }
 parallel of declination at 3^h } 7 5 13 decreasing 53" per hour:
 P. M.

The latitude of the observatory, as }
 above laid down } 40 9 56

Hence the parallaxes were fitted to each of the
 micrometer observations, as above.

If a computation be made with the 1st micrometer obser- }
 vation of the distance of the limbs, we shall find the }
 time of the least distance of the centers of the Sun and } 5 26 16—
 Venus, as seen from the earth's center, to have been }

the Sun and center of Venus to the cross-hairs of the equal alti-
 tude instrument; but it was found that so many lines would
 confuse the figure. And the micrometer observations answering
 so well, more were thought needless. Besides this, no fractions
 of seconds could be got in the other observations; though, ne-
 vertheless, a good separate projection may be made by them.

If a like computation be made from the 16th observation, it will be found	h ' "	5 26 21
By comparing some other observations with these, I conclude, the time of the least distance of the centers was	h ' "	5 26 20
Then as radius to tangent of the angle of the ecliptic, with path of Venus	o ' "	8 28 27
So is the least distance of the centers		610 9,1731571 2,7853298
To the distance of Venus's place, when nearest the Sun's center, from her place at the time of ecliptic conjunction		90,88 1,9584869
Which reduced to time (to be subtracted) From time of the least distance of the centers	h ' " viz.	o 22 41 5 26 20
Leaves for the time of ecliptic conjunction		5 3 39
As radius to secant of So is	o ' "	8 28 27 10,0047676 610 2,7853298
To the geocentric latitude of Venus at the time of ecliptic conjunction		616,73 2,7900974
From the logarithm of which subtract the difference of the logarithms of the distance of Venus from the Earth and from the Sun		o,4002370
Remains the logarithm of the heliocentric latitude	' "	= 4 5,39 2,3898604
As tangent of inclination of Venus's orbit To radius; so is tangent of Venus's helio- centric latitude	o ' "	3 23 20 8,7724442 o 4 5,39 7,0754375
To the sine of her distance from node in the ecliptic		1 9 4 8,3029933

The

	S	°	'	"
The Sun's place at the time of the ecliptic conjunction (by Halley's Tab.) was	}	2	13	26 32
Add the distance of the node from the Sun		0	1	9 4
The sum is the place of the ascending node of Venus		2	14	35 36
The place of Venus, by Halley's Tables, to the same time		8	13	26 22

‡ That is, ten seconds too little.

In order to find the error of the micrometer (if any), Jupiter's diameter was measured to the right and to the left; and Mr. Rittenhouse afterwards took the trouble to measure the diameter of a white painted circle both ways ten times. This work was performed early in the morning before sun-rise, when the air was still, and free from all tremulous motion; the result of which, on a mean of those to the right, and a like mean of those to the left, was an error of adjustment for the micrometer of $1''.12$ to be subtracted; which was accordingly allowed for in the reduction of all the micrometer measures.

Thus we have given a full and faithful account of our work. We could have wished to have comprized it in less room. Had our latitude and longitude been well fixed, as they had been at Philadelphia, by able mathematicians, beforehand, a considerable part of our work might have been saved. But as it was necessary to shew, that such pains have been taken in these material articles, that they may be depended on; and as we had opportunities

portunities of observation, from the goodness of the weather, and other circumstances, which cannot have happened to the generality of observers in many parts of the world, we thought we should be the easier excused by men of science, for the insertion of twenty superfluous things, than the neglect of any thing material in the account of a phenomenon, which will never be observed again by any of the present generation of men.

I am,

GENTLEMEN,

with great respect,

Your most obedient,

humble servant,

Philadelphia, July 19,
1769.

William Smith.

N. B. Fig. 5, plate XV, represents the appearance of Venus on the Sun to the Reverend Mr. Hitchens, at the Royal Observatory. See *Philos. Transf.* vol. LVIII. for 1768, p. 363.

XLII. *Observationes Transitus Veneris per Discum Solis, Die 3 Junii, 1769, habitæ in Suecia, et Societati Regiæ Londinenfi communicatæ, a Petro Wargentin, ejusdem Societatis Sodali, et Academiæ Regiæ Scient. Stockholmenfis Secretario. Communicated by Mr. John Ellicott, F.R.S.*

Read Dec. 16, 1769. **T**RANSITUS Veneris per discum Solis, qui contigit die 3 Junii hujus anni, novam et exoptatissimam astronomiæ præbuit occasionem limatius determinandi veram quantitatem parallaxeos Solis horizontalis, si modobonæ observationes justis locis habitæ sint. Et cum *Suecia* inter ea terræ loca fuerit, ubi observationes habitæ, collatæ cum aliis in *Mari Pacifico*, vel *America*, institutis, magni essent ponderis, suo officio deesse nequitiam voluerunt astronomi *Surci*, quominus, suo quisque loco, ad rarissimum hoc phenomenon debita cum diligentia attenderet. Sed quoniam et immersio et emerſio Planetæ, non nisi in borealibus regni partibus videri potuerit, Academia R. Scientiarum *Stockholmenſis* omnino necessarium duxit, mittere duos observatores exercitatos et bonis instrumentis instructos, in *Laponiam*, qui diversis locis transitum hunc opperirentur. Alter, Dominus Mallet,

Mallet, Astronomiæ Observator Regius *Upsaliæ*, villam *Pello* adiit, quæ terminus borealis fuit arcus meridiani terrestres, ab astronomis *Parisiensibus*, anno 1736, mensurati. Alter, Dominus *Planman*, Philosophiæ Naturalis Professor in Academia *Aboensi*, *Cajaneburgum* repetiit, oppidum in confiniis *Finlandiæ* et *Laponiæ* situm, ubi priorem Veneris per Solem transitum, anno 1761, feliciter notaverat. Ista stationes Academia *Stockholmensis* eam præcipue ob causam delegerat, quod earum situs geographicus jam antea quadantenus determinatus fuerit. Nisi enim longitudo loci certa sit, observatio per se optima solius immersionis vel emersionis Veneris irrita fere esset: raræ autem sunt occasiones determinandi longitudes locorum, præsertim tempore verno et æstivo, prope circulum polarem arcticum, ubi eo tempore continua lux Jovem videre et fixarum à Luna distantias observare vix permittit. Eclipsis quidem Solis, quæ die 4 Junii incidit, videbatur bonam offerre ansam determinandi meridianorum differentias; sed præterquam quod fieri posset, ut qui immersionem vel emersionem Veneris feliciter observavit, cælum nubilum haberet sub eclipsi Solis, notum quoque est, observationes correspondentes eclipsium Solis non satis certas dare meridianorum differentias. Academia itaque prudentius agere sibi visa est, eligendo loca stationum ante determinata, quamvis Sol, tempore transitus, minus ibi esset elevatus, quam, pro obtinenda parum majori Solis altitudine, loca magis borealia sed indeterminata præscribendo.

Præter dictos duos observatores, tertius, Dominus *Hellant*, ipsius urbis *Torneæ* incolæ, et qui per 30 annos varias observationes astronomicas ibi dextre instituit,

instituit, suam hac quoque occasione operam pollicitus est.

Incidente demum die 3 Junii, D. *Mallet* optimam spem habuit bonas nanciscendi observationes, coelo enim sereno gavissus est per totam fere diem, immo etiam per noctem sequentem, sed nubes circa horizontem borealem omnes ipsius conatus observandi momenta immersionis et emersionis Veneris, prorsus irritos, proh dolor! reddiderunt. Sub ipso transitu Planetam in Sole aliquoties vidit, ejusque diametrum situmque in disco, ope micrometri objectivi, mensus est; sed harum observationum copiam mihi nondum dedit. Totam quoque Solis eclipsin bene a se observatam esse narrat, sed hoc debile nimis solatium invisæ Veneris, et vile pretium tanti itineris jure putat.

Hellantius Torneæ, Venerem adhuc minus propitiam expertus est. Coelum enim hisce diebus fere continuè nubilum habuit, ut ne quidem eclipsin Solis observare illi licuerit.

Planmannus autem *Cajaneburgi* felicius fuit. Ibi coelum tota die præcedente densis nubibus fuerat obductum, quæ omnem observationum spem exstinxerant, sed inopinato dehiscabant nubes circa Solem, vesperi, hora 9 8', tum vero Venus jam cœperat Solem ingredi, et tertia circiter diametri sui parte intra discum Solis erat. Hora 9 20' 45'' $\frac{1}{2}$ tota immergebat, eo enim momento rupta est subito fasciola, quæ huc usque Solis et Veneris margines junxerat, et Planetæ corpus lumine Solis undique est circumfusus. Paucis post hoc momentum minutis, redibant nubes, quæ noctem largis imbribus et tonitribus fulminibusque horridam fecerunt, tristemque astronomo nostro, qui

spem ulterioris observationis amiserat. At horâ 3 21' matutinâ, resplenduit Sol, nimis licet fero, Veneris enim pars tertia circiter jamjam emerferat. Itaque tanto majori cum attentione emerfionem totalem vel contactum limborum exteriorum exspectabat: contigit ille, horâ 3 32' 27'', Sole tum splendido. Dolendum quidem est, cœli faciem non indulfisse *Planmanno*, contactum interiorem emerfionis tueri; sed cum exteriorum bene et certè, tubo 20 pedum optimo notatum afferat, et per experientiam transitus anni 1761, compertum sit, observationes contactus exterioris in emerfione fere magis inter se convenire et parallaxeos quantitatem non minus certam dare, quam observationes correspondentes contactus interioris, sperare fas est, hanc observationem celeb. *Planmanni*, comparatam cum aliis, eximio futuram esse usui, si aliis astronomis in *Norvegia* et *Russia* non contigit (quod tamen abfit) emerfionem Veneris totam observare.

		h ' "
Die 4 Junii, eclipses Solaris initium, cœlo sereno, notavit	}	9 0 53
D. Planman		
Finem		11 0 0 $\frac{1}{2}$

Elevatio Poli *Cajaneburgensis* est $64^{\circ} 13' \frac{1}{2}$. Differentia meridianorum observatorii Regii *Grenovicensis* et *Cajaneburgensis* est $1^h 50' 47''$ quam proxime, quantum ex habitis anno 1761 observationibus concludere licet, quam etiam confirmare videntur nuperæ observationes immerfionis Veneris, *Cajaneburgi*, *Upsaliae*, et *Stockholmiae* factæ, atque inter se collatæ.

OBSERVATIONES UPSALIENSES.

Tota die 3 Junii, cœlum *Upsaliæ* fuit serenissimum: nihil itaque aliud obstitit certitudini observationum immersionis Planetæ, quam undulatio marginis Solaris, quæ, in elevatione duorum vel trium tantum graduum ab horizonte, non potuit non esse magna et incommoda.

Upsaliæ quinque observatores in immersionem Veneris intentos habuere oculos animosque.

Dominus <i>Stroemer</i> , Astron. Professor emeritus, usus est telescopiorefflectente trium pedum.		h ' "	
Is Solis, Venerisque margines se interius tangentes observavit	}	8	39 58
Sed eos separatos et obscuram fasciolam eos jungentem ruptam demum vidit		8	40 32
Dominus <i>Melander</i> , Astron. Professor, tubo 20 pedum, Veneris accessum primus notavit	}	8	22 1
Margines Solis et Veneris se intus contingebant		8	39 57
Fasciola margines jungens illi visâ est rupta		8	40 12
Dominus <i>Bergman</i> , Chemiæ Professor, tubo 21 pedum dictam fasciolam ruptam vidit	}	8	40 9
Dominus <i>Prosperin</i> , Astron. Observatoris Vicarius, tubo 16 pedum, Venerem exterius tang. vidit		8	22 12
Immersionem totalem, rupto vinculo obscuro, observavit		8	40 12
Dominus <i>Salenius</i> , Philosophiæ Magister, tubo 12 pedum, primum Veneris in margine Solis vestigium aspexit	}	8	22 15
Venerem, undique Solis lumine cinctam, videre sibi visus est		8	39 46
Sed mox disparuit illa lux, et Solis margo cum Veneri iterum coalescebat, vinculo vel fasciola atra coherentes, quæ demum rupta	}	8	40 15

Postremi hujus phænomeni duplicis scilicet immersionis totalis, explicationem ingeniosissimam dedit clariss. *Melander*, quam alia occasione communicabo.

OBSERVATIONES STOCKHOLMENSES.

Heic non tantum undulatio marginum Solis, sed et nubeculæ prope horizontem, reliquo licet cœlo adhuc nitido, anxios nos reddiderunt de observationum successu, qui tamen spe melior factus est, enitescente margine Solis faltem superiori, iis momentis, quibus maxime opus erat.

Dominus <i>Ferner</i> , Cancellariæ Regiæ jam Confiliarius, et olim Astron. Professor, tubo Dollondiano 10 pedum, diametros siderum nonagies augente, primum Veneris in margine Solis vestigium vidit	} h ' "	8 24 8
Cornua Solis Planetam amplectentia confluebant, ideoque imm. total.		
Dominus <i>Wilcke</i> , Physices Experim. Lector, telescopio 1 $\frac{1}{2}$ pedum egregio, appropinquantem Venerem in ipso Solis margine conspexit	} 8 41 48	8 24 5
Margines Solis et Veneris se fere contingere videbantur, quamvis cohærent		
Fasciola margines connectens diluebatur paulatim, conspicua tamen adhuc erat	} 8 41 2	8 41 30
Eadem fasciola, tenuior et minus obscura sensim facta, evanuit, et Venerem intra Solem reliquit		
Ego, tubo 21 pedum, inter innumeras fluctuantes inæqualitates in margine Solis, constantem tandem et atram detexi (quam mox ipsissimæ Veneris esse noram cognovi)	} 8 41 45	8 23 51
Margines se interius ad sensum tangere videbantur		
Cohærebant tamen, donec radius Solis fluctuans eos separabat	} 8 41 32	8 41 47

Ab ortu Solis proximo mane, usque ad horam 8 $\frac{1}{4}$ matutinam, cœlum nubibus fuit obductum, et *Stockholmiæ* et *Upsaliæ*.

Stockh. d. 7 Jul.

1769.

XLIII. *Observations of the Transit of Venus over the Sun, contained in a Letter to the Reverend Nevil Maskelyne, Astronomer Royal, from Dr. Alexander Wilson, Professor of Astronomy in the University of Glasgow.*

College, Glasgow, Sept. 9, 1769.

SIR,

Read Dec. 7, 1769. **I** SEND you now the particulars of my observation of the transit of Venus, together with the observations of those who acted in concert with me. I chose for the place of my observations a house at some distance from our observatory, but in sight of it, and more free from the smoke of the town; where I had two gentlemen to attend the clock, and mark the times. I carried with me two reflectors of Mr. Short's, which are described below. Three other instruments were made use of at the observatory; the first was an achromatic tube of Dollond's, 29 inch focus, by which an image of the Sun was formed, of about six inches diameter, on a board covered with paper. The telescope being mounted upon a frame, by which it could be turned about as the Sun moved, and the room properly darkened. This instrument was managed by Dr. Williamson
and

and Dr. Reid, at the west window of the room of the observatory, wherein the astronomical clock stood; the other two instruments were placed without, at the south and north windows of the same room, one being a refractor of 13 feet, by which Dr. Irvine observed; the other a 12 inch reflector of Short's, by which my son observed. These two observers looked directly at the Sun, having their instruments armed with smoke-glasses; another person stood at the clock, and counted the seconds by coincident beats upon a piece of board, which he held in his hand for that purpose, and who named every fifth second, so that all the observers could hear him distinctly. The motion of the clock, made by Shelton, was carefully adjusted by many transits of the Sun and fixed stars, over the meridian, both before and after the day of the transit; the clock by which my observations were made was adjusted by Shelton's, by means of signals made every hour, for some hours before and after the transit. It was apprehended, that the smoke of the town might hurt the observations; and, to prevent this as much as possible, an advertisement was put in the news-paper, begging the inhabitants, in cases where it would not be very inconvenient, to put out their fires from three o'clock that afternoon till sun-setting; the politeness of the inhabitants of Glasgow, in complying with this request, was far greater than could well be expected, insomuch that there was not a spire of smoke to be perceived in that quarter from which the observations could be incommoded. Having made these preparations, we thought we had nothing to fear but the clouds; and indeed the western part of the heavens was covered with

with thick clouds all the afternoon, till a short time before the external contact; but they drove away towards the north, and left the Sun perfectly bright, excepting that now and then a cloud passed over him. But we soon found that the constitution of the air was otherwise unfavourable to our observations; the image of the Sun on the white board, made by the achromatic telescope, was bright enough; but there was a remarkable undulation in the limb, which could be owing to nothing else but the state of the air. This inconvenience was also sensibly felt both by the other gentlemen and myself. Besides the undulation now mentioned in the limb of the Sun, there was also a considerable tremor round the planet Venus, when she was seen upon the Sun's disc, and, in consequence of this, an indistinctness in her limb, which made it impossible to measure her diameter by our object glass micrometer, or otherwise. After the center of Venus had passed the Sun's limb, she appeared to us not to be circular, but oblong, the longest diameter being that which passed through the Sun's center. As the internal contact approached, Venus appeared to us to adhere to the Sun's limb, by a dark protuberance or neck, both the length and breadth of which varied every moment by a constant undulation: neither did this neck break off instantaneously, but changed its colour from black to a dusky brown, till at last the interval betwixt Venus and the Sun's limb appeared quite clear. Each of the observers wrote down his observations on the spot. I reduced them, together with my own, to apparent time, from the observations I had

had made on the going of the clock, and arc as follows:

		h	'	"
By Doctor Watson.	External contact	6	54	31,4
	Venus's center judged to be on the limb	7	1	33,4
	Sun's light appeared betwixt Venus and the limb	7	11	56,7
	The beginning of the Solar eclipse next morning, observed by Short's 18 inch reflector	18	30	14,2
	Middle, from a series of observations with the object glass micrometer, fitted to a nine inch reflector of Short's	19	18	47,7
	End not visible			
By Dr. Wil- liamson and Dr. Reid.	External contact	6	54	28
	Internal contact, or when the Sun's light appeared betwixt Venus and the limb	7	12	24
	Venus's center judged to be on the limb, by Dr. Reid	7	1	24

Dr. Reid marked the time when he conceived the internal contact would have happened, if the dark protuberance upon Venus had been taken away, and her disc reduced to a circle, viz. $7^h 10' 24''$.—He thinks it likewise proper to mention, that, several seconds before the time above set down as the time of the internal contact, he saw a small dint upon the Sun's limb, which he took to be the external contact; that he immediately mentioned this to Dr. Williamson, who happened that instant to have his eyes turned another way; but before he could look at it, it disappeared by the undulation in the Sun's limb. Dr. Reid is the more persuaded that this was not mere imagination, because this dint on the limb of the Sun appeared to him much nearer to the Sun's vertical

vertical diameter than he expected it, but in the very point, however, where it was clearly seen immediately after.

		h	'	"
P. Wilson.	{	External contact	6	54 28
		Internal contact	7	12 24

My son desires me to remark, that his first observation should be considered as no other but a posterior confirmation of Dr. Williamson's and Dr. Reid's external contact; the fact was, that when these gentlemen perceived the first contact, their keenness made them call out, and it was not till then that he saw the phenomenon with perfect certainty. He was conscious, however, that he fluctuated concerning the reality of the appearance for about twelve seconds before that time, during which his determinations were suspended, through an apprehension of anticipating the real time, which was heightened by so close a neighbourhood with the other observers, all of whom he could not help being sensible were still expecting the phenomenon. Upon the whole, he is rather of opinion that he would have put down the external contact at least eight seconds sooner had he been observing apart. His second observation, by which he means the instant when the interval between Venus and the Sun's limb first appeared obvious, was taken down without the least knowledge of what was passing among the other gentlemen who observed. Dr. Irvine has been out of town for some considerable time past, and forgot to lodge his observations with me, but I remember certainly that he made the external contact three seconds sooner than the rest; but his internal contact was some seconds

later, but how many I do not now remember. Mr. Anderson, F.R.S. fitted up a clock and apparatus in the college steeple; his clock was regulated as above, by signals from the observatory; he observed the transit with a large reflector, and his assistants observed with refractors: they were all of them uncertain about the external contact, owing to the state of the atmosphere, and a tremor given to the steeple by the wind; but none of their other observations varied, above three seconds, from my own, as related above.

	° ' "	
Latitude of the observatory	55 51 32	
Longitude by corresponding observations	} 0 17 11	of time from Greenwich W.

I am, SIR,

with great esteem,

Your most obedient servant,

Alexander Wilson.

I have observed, during the course of this year, several times, the Aurora Borealis form itself into an arch, 20 or 30 degrees above the horizon, which continued permanent for some time; and in this case the vertex of the arch appeared always to be west from north, by about the variation of the needle in 19 or 20 degrees. Whether or not it is always so, I cannot yet say.

In the internal contact, James Hoy differed from the other gentleman and me two minutes, he calling it 12 minutes, and we 14 minutes; which of us is wrong, will be no difficult matter to determine. In the internal contact we all observed the black ligament or protuberance, which was not broke for some seconds after the regular circumference of Venus seemed to be within the Sun; and the observation we send you was, as near as we could judge, about the time this protuberance was going to break. Lord Alemoor also, and he only, observed regular circumferences of the Sun and Venus in contact, at $7^h 14' 10''$, mean time.

The morning promised ill, yet we got 9 very good altitudes of the Sun near the prime vertical. About noon the day was terrible, with thick clouds, and like settled rain. You may imagine how we felt. About two o'clock the wind began to change from the south to the westward; about three o'clock it was west, and the clouds breaking; so that we got 5 very good corresponding altitudes. There was, about 4 o'clock, a very hard thunder shower, and calm, after which the wind began to blow briskly from the north-west; the clouds blown away, and those near the horizon depressed and held down, the Sun shone clearer than I ever saw it, and not a cloud was to be seen in that quarter. It remained so till after both contacts; when, not half a minute after, small flying clouds passed over the Sun, and shewed us how much we were obliged to kind heaven for the very favourable opportunity we had of making our observations. It appeared, I assure you, as if Providence had withdrawn the clouds over head, and held down those near
the

the horizon, for that very purpose. The night continued equally clear and serene, as did the morning, till after the eclipse; half an hour after which it began to overcast, and put on the same cloudy appearance it has wore for some months past. Although the morning was so favourable, yet we lost the beginning of the eclipse, from being too long in getting to our posts; however, I here send you the contacts, with the different spots of the Sun, and its end most exactly. If you observed the spots, it will, I imagine, be as exact as if the beginning and end only had been observed.

I am,

SIR,

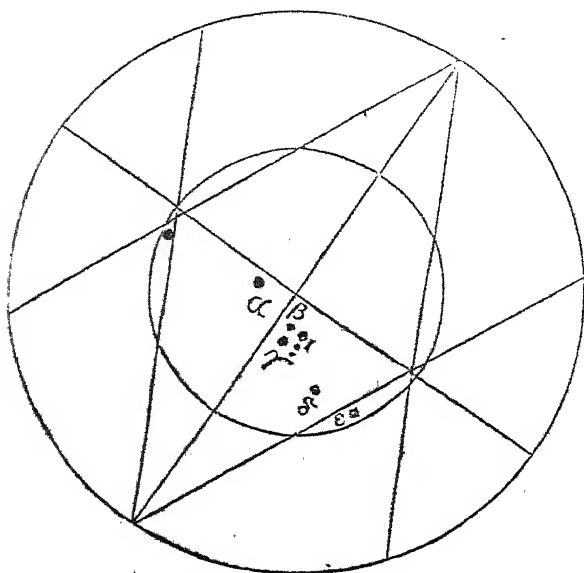
Your most obedient,

humble servant,

James Lind.

P. S. I hear the day proved also very favourable at Glasgow.

Mean times of the contacts of the Moon's limbs, with
the spots of the Sun, June 3, 1769.



		Mean time.		
		h	m	s
α's S. E. limb with	α { Ext. contact	18	47	33
	α { Center	18	48	1
	α { Int. contact	18	48	18
	β { Center	18	59	20
	γ { Ditto	19	1	18
	δ { Ditto	19	9	9
	ε { Ditto	19	18	23
α's W. limb with	α { Int. contact	19	37	18
	α { Center	19	37	47
	α { Ext. contact	19	38	15
	β { Center	19	51	16
	γ { Ditto	19	52	47
	δ { Ditto	20	3	46
	ε { Ditto	20	15	9
End of the eclipse		20	17	30

Remarks

Remarks by the ASTRONOMER ROYAL.

Hawkhill is said by Dr. Lind to be about $1\frac{1}{2}$ miles N.E. of Edinburgh. It is the seat of Lord Ale-moor, one of their judges, who is fond of astronomy, and has built a small observatory there with a moveable roof, upon Mr. Smeaton's plan, which I sent to Dr. Lind. The corresponding altitudes, for determining the time of the observations of the transit of Venus, were taken, by reflection, from a basin of quicksilver or treacle, with a brass Hadley's sextant, made by Mr. Ramsden; the surface of the fluid being defended from the wind by a glass ground truly plane. They find that the equal altitudes seldom differ above two or three seconds in determining the time of noon; so that, by taking a great many at once, and taking the mean, they think they cannot fail of coming very near the truth. I have examined the equal altitudes made about the time of the transit, and the times of the contact are given corrected in the foregoing account. The clock in the observatory seems to go pretty well, though it only beats dead quarter seconds; it has a mahogany pendulum, and was made by Mr. Cummins. In the house was a clock beating seconds, and set, by means of the other, in the afternoon, before the beginning of the transit. The latitude of the place was also determined by meridian altitudes, taken by reflection with the sextant, and, by the mean of 10 observations, which all agree within the compass of .2 minutes, is $55^{\circ} 57' 37''$ N. The end of the Solar eclipse was
 observed

observed by two persons with the two achromatic telescopes, with treble object glasses, and they agreed to a second.

Dr. Lind writes, another time, that, being from home, at Lees, near Coldstream, 7 miles west of Berwick, he observed the latitude of the place about $55^{\circ} 37'$.

The foregoing particulars are extracted from letters received from Dr. Lind. He has also communicated to me the following observations of the transit of Venus and Solar eclipse, made by the Reverend Mr. Brice, at Kirknewton, as follows :

I here likewise send some observations, made in our neighbourhood, by the Reverend Mr. Bryce, Minister at Kirknewton. He is a very good astronomer, and is a writer in the Philosophical Transactions. Kirknewton is in lat. $55^{\circ} 54' 30''$ N. and about 17 miles W. of Hawkhill, from measuring it on Lawrie's map of the environs of Edinburgh.

The clock had been tried by several transits of a fixed star, and always found to measure time so exactly, that in the space of five days it did not differ one second from the truth ; it was also examined by taking equal altitudes of the Sun, and found to be $18''$ slow. The day was cloudy, with flying showers, till about two o'clock in the afternoon ; then it grew somewhat clear, and about four the Sun shone out exceeding bright, when I observed carefully the spots upon the Sun ; the brightness continued till about $15'$ before 7^h , when a cloud came over the Sun, which was not seen till $6^h 55' 40''$ mean time, as shewn by the clock, and then Venus had made a sensible

sensible impression upon the upper limb of the Sun's zenith, and $\frac{1}{10}$ th, as I judged, upon the Sun.

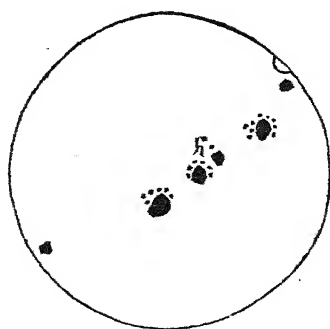
Half on the Sun, as we thought,
Internal contact clearly seen
18'' added for the clock too slow

h	'	"
7	3	55
7	11	55
	+	18

And if Venus takes 19' from the first impression to the internal contact, the transit began at }
Seen going till the Sun set in a cloud near the horizon } 8 24 39

When near the horizon, Venus's edge was full of notches and protuberances, and she appeared as if moving round like a wheel.

Eclipse of the Sun, 4 June, common reckoning.



- (1) Beginning of the eclipse
- (2) The 1st impression made a little above the line of the spots upon the Sun's disk }
- (3) The Moon's edge touches the great spot
- (4) 1st cluster of spots touched
- (5) Covered at
- (6) Cusps of the Sun upon an horizontal line
- (7) Another spot touched
- (8) Spot on the Sun's north limb touched
- (9) 1st spot of the cluster emerged

h	'	"
6	27	50
6	56	5
6	57	0
7	0	0
7	6	45
7	7	25
7	16	44
7	50	20
	(10)	2d

- | | | |
|------|---|---------|
| (10) | 2d spot of the cluster emerged | 7 50 40 |
| (11) | 3d spot of ditto emerged | 7 51 32 |
| (12) | Spot on the north limb emerged | 8 13 40 |
| (13) | Eclipse ended | 8 15 50 |
| (14) | Last impression made at the spot upon the north limb of the Sun,
and the whole eclipse seen very distinctly, from the beginning to
the end. | |
| (15) | An evident irregularity in the under edge of the Moon, which
entered upon the south side of the Sun, and traversed it from
south to north. | |
| (16) | Fahrenheit's thermometer stood all the while, in the shade, at $53^{\circ}\frac{1}{4}$,
and continued so for several hours after. | |

N. B. The clock 18'' flow to be added to the several observations.

N. B. The internal contact is, when the thread of light was compleated.

From the above observation, and from every one of any credit, we Hawkhill gentlemen are late in the external contact.

XLV. *Observation of the Transit of Venus, and other Astronomical Observations, made at Gibraltar; contained in a Letter to the Astronomer Royal from Lieutenant Jardine.*

To regulate the clock.

Read Dec. 7, 1769. **A**N equal altitude instrument was fixed (nearly such as is described in Smith's Optics, vol. II. p. 328), on which was mounted a small telescope with cross hairs.

		Sun's upper limb touched the ho- rizontal hair at			Sun's lower limb touched the ho- rizontal hair at		
		h	'	"	h	'	"
June 1	Morning	8	32	43	8	35	8
	Afternoon	3	24	33	3	22	0
2	Morning	8	32	20	8	34	46
	Afternoon	3	25	4	3	22	36
4	Morning	8	32	27	8	35	13
	Afternoon	3	25	27	3	22	47
5	Morning	8	32	35	8	35	10
	Afternoon	3	25	53	neglected		

TRANSIT OF VENUS.

By three observers, with two $7\frac{1}{2}$ feet refractors, and one 2 feet reflecting telescope.

	h	'	"
June 3, Venus's 1st external contact with the Sun, at	6	49	58
1st internal contact with the Sun, at	7	7	11
Sun set behind a hill	7	8	3
Clock before mean time	0	1	8,8

Y y 2

For

For the LATITUDE of the place.

Observed by a Hadley's quadrant, divided into minutes, the double meridian altitudes of Jupiter and Cor Scorpii, reflected from water.

Double meridian altitude of Jupiter.

		°	'	"
May	30	=	77	6
June	14	=	77	50
	17	=	77	55 30
	21	=	78	3 30
By another observer,		=	78	3 0
June	22	=	78	4 0
	29	=	78	10 45
July	4	=	78	14 0

Double meridian altitude of Cor Scorpii.

		°	'	"
June	28	=	56	7
	29	=	56	5
July	7	=	56	6 30
	8	=	56	5 0

Clear weather, in general, during these observations; and Fahrenheit's thermometer, in the middle of the day, between } 75 and 80
in the night, between } 68 and 71

For the LONGITUDE of the place.

		h	'	"
May	30	Emerfion of Jupiter's first fatellite	12	59 56
		Clock before mean time	0	1 2
May	31	Emerfion of Jupiter's 2d fatellite	10	51 51
		Clock before mean time	0	1 3 1/2
June	8	Emerfion of Jupiter's first fatellite	9	22 34
		Clock before mean time	0	1 16 1/2
June	15	Emerfion of Jupiter's first fatellite	11	15 54 1/2
		Clock before mean time	0	1 28
June	25	Immerfion of Jupiter's 3d fatellite	11	59 56
		Clock before mean time	0	1 44
July	8	Emerfion of Jupiter's first fatellite	11	30 57
		Clock before mean time	0	2 4

These two are most to be depended upon.

{ uncertain to perhaps 5 or 6"

ECLIPSE

ECLIPSE OF THE SUN.

			h	m	s	
June 4	First contact at	6	6	54	seen perhaps a little too late.	
	Last contact at	7	19	28	exact.	
	Clock before mean time	0	1	9		

Elev. of ☉'s l. limb by Hadley's quad. at $\left\{ \begin{array}{l} \text{1st contact } 14 \text{ } 41 \\ \text{2d contact } 28 \text{ } 55 \end{array} \right\}$ both exact.

Dip of the horizon, for 160 feet above the level of the sea, is to be allowed.

SIR,

I Have been disappointed in the pleasure I promised myself, when I saw you, of observing some occultations of fixt stars, by the Moon, &c. We shall be glad, if these observations can be of any service. We have endeavoured to discover to you the degree of dependance to be placed thereon.

With regard to the clock, we conclude from these equal altitudes (correcting for difference of declination, &c.), that, on the 3d of June, it was before mean time 1' 8'',8. You will easily discover if there is any error. We afterwards regulated by frequent equal altitudes, by a meridian line on the bottom of a window, and by the setting of stars behind some solid buildings.

The latitude appears, from these double altitudes, to be somewhere between $36^{\circ} 3'$, and $36^{\circ} 4'$; but from more correct declinations, &c. you will be able to determine it more precisely.

For the eclipse, we had no micrometer, nor any other method of determining the quantity of it.

I To

To the eye, between $\frac{1}{4}$ th or $\frac{1}{3}$ th of the Sun's diameter seemed to be eclipsed. Though the beginning be rather incorrect, the end may be depended on.

We shall be glad if you can determine, from hence, the latitude and longitude of the place, and to know the result of the discoveries made in our system, by the observations of the late transit, if you will favour us.

I am,

S I R,

Your most obedient,

humble servant,

Gibraltar, July 14,
1769.

Alexander Jardine.

By re-computing these observations, I find, that the external contact of Venus happened at $6^h 51' 8''$, the internal contact at $7^h 8' 21''$, the beginning of the eclipse of the Sun at $18^h 8' 0''$, and the end at $19^h 20' 33''$, all apparent time; and that the latitude of the place, by the mean of the 4 altitudes of Cor Scorpii, is $36^\circ 4' 44''$, N. The dip of the horizon of the sea, for an elevation of 160 feet, may be reckoned $12' 5''$.

Nevil Maskelyne.

XLVI. *Observations of the Transit of Venus over the Sun, June 3, 1769. In a Letter to the Reverend Nevil Maskelyne, F. R. S. Astronomer Royal, from John Winthrop, Esquire, F. R. S. Hollisian Professor of Mathematics at Cambridge, in New England.*

Cambridge, New-England, Sept. 5, 1769.

REVEREND SIR,

Read Dec. 7, 1769. I BEG leave to lay before you my observation of the late transit of Venus, the beginning of which I observed, in this place, with all the care I was capable of. Our situation on this North American coast, I am sensible, was not the most favourable, as we could see only the first part of the transit; yet, I presume, careful observations, wherever made, will have their use in determining the grand problem of the Sun's parallax; at least, may serve as collateral evidences to the capital observations.

Our apparatus having been wholly destroyed by fire, about five years ago, we have since procured a new set of astronomical instruments, made by some of the most eminent hands in London. A clock, by
Mr.

Mr. Ellicott, with the pendulum of his invention, having the bob supported by levers. An astronomical quadrant, of 2 feet radius, made by Mr. Siffon; and an equal altitude instrument, by Mr. Bird; each having three horizontal wires in the telescope. A reflecting telescope, of four feet focus; another of two; and another of 1 foot; fitted with an object glass micrometer, of $21\frac{1}{2}$ feet focus; all three made by the late Mr. James Short.

I was obliged to remove the clock to another apartment, for the sake of the transit, which I did on the 23d of May, when I took some equal altitudes. By reason of an almost continual succession of cloudy weather till the end of that month, I could make but few material observations afterwards for regulating the clock. But, happily, the weather cleared up on the 1st of June, about noon, and continued fine for several days. As the precise knowledge of the true time is of the utmost consequence in the present case, I suppose a particular account of the observations made for this purpose, within a few days of the transit, will be most satisfactory to you; and this must be my apology for the prolixity of the detail.

Observations for regulating the clock.

1759

June

24

Very cloudy till noon; the Sun hardly visible for a minute at a time; so that I could make only the following sparse observations.

Equal altitudes of the Sun's limbs.

Morning.			I. Afternoon.			Middle Times			☉ on the meridian.			
h	'	"	h	'	"	h	'	"	h	'	"	
7	55	13	4	0	6	11	57	39.5	I.	11	57	35.3
						equation—4.2			II.			35.6
						11	57	35.3	III.			35.2
						☉ on the meridian						
8	13	36		41	43	—11	57	39.5	Mean	11	57	35.4
14	6		3	41	13	equation—3.9			or, true time of noon			
						11	57	35.6	by the clock.			
						☉ on the meridian						
						11 57 39						
						equation—3.8						
						11	57	35.2	☉ on the meridian			

2

			I.					
☉'sup. limb	1	8	3	39	51	49	44	
	2		4	49	50	38	43.5	
	3		6	0	49	28	44	
low. limb	1		6	30	48	58	11 57 44	
	2		7	41	47	46	43.5	
	3		8	52	3	46	35	43.5
			Mean			11	57	43.75
			Equation			—3.77		
			☉ on meridian			11	57	40

			II.					
☉'sup. limb	1	8	11	47	43	41	44	
	2		12	58	42	31	44.5	
	3		14	9	41	19	11 57 44	
low. limb	1		14	39	40	49	44	
	2		15	50	39	38	44	
	3		17	1	3	38	27	44
			Mean			11	57	44.1
			Equation			—3.7		
			☉ on meridian			11	57	40.4
			Z z					

1769
June
2

		Morning.			Afternoon.			Middle times.		
		III.								
		h	'	"	h	'	"	h	'	"
☉ sup. limb	1	8	35	42	19	45		43	5	
	2		36	54	18	33		43	5	
	3		37	25	18	2	11	57	43	5
low. limb	1		38	37	16	51		44		
	2		39	48	3	15	40	44		
	3									
		Mean			11 57 43.7					
		Equation			-3.3					
		☉ on meridian			11 57 40.4					

		IV.						☉ on the meridian.		
		h	'	"	h	'	"	h	'	"
☉ sup. limb	1	8	41	49	13	37		43		
	2		43	1	12	25		43		
	3		44	13	11	14	11	57	43	5
low. limb	1		44	45	10	43		44		
	2		45	57	9	30		43	5	
	3		47	10	3	8	17	43	5	
		Mean			11 57 43.4			Mean of all 11 57 40.3		
		Equation			-3.2			or, true time of noon by the clock.		
		☉ on meridian			11 57 40.2					

		I.								
		h	'	"	h	'	"	h	'	"
☉ sup. limb	1	7	46	28	9	12				
	2		47	39	8	1	11	57	50	
	3		48	9	7	31	equat.	-3	8	
low. limb	1		49	20	4	6	20			
	2									
	3									
		☉ on meridian			11 57 46.2					

		II.								
		h	'	"	h	'	"	h	'	"
☉ sup. limb	1	7	51	38	4	2		50		
	2		52	48	2	51		49	5	
	3		53	59	1	40	11	57	49	5
low. limb	1		54	29						
	2		55	40	4	0	0	50		
	3		56	50						
		Mean			11 57 49.75					
		Equation			-3.74					
		☉ on meridian			11 57 46					

1769
Jan.
E.

Morning. Afternoon. Middle times.

III.									
h m s			h m s			h m s			
☉ sup. limb	1	7 58 51	56 46			48,5	☉ on the meridian.		
	2	8 0 2	55 35			48,5			
	3	1 13	54 34	11 57	48,5				
low. limb	1		53 55				I. 11 57 46,2		
	2	2 54	52 46		50		II. 46		
	3	4 5	3 51 35		50		III. 45,5		
Mean						11 57 49,1	Mean of all 11 57 45,9		
Equation						-3,6	or, true time of noon by the clock.		
							At 4 ^h 15' thermometer 85½.		
☉ on meridian						11 57 45,5			

I.								
h m s			h m s			h m s		
☉ sup. limb	1	7 45 1	10 51		56	☉ on the meridian. I. 11 57 52,4 II. 56 III. 55,5		
	2	40 12	9 40		56			
	3	47 23	8 29	11 57	56			
low. limb	1	47 53	7 59		56	Mean of all 11 57 56- or, true time of noon by the clock. At 4 ^h 15' thermometer 85 $\frac{1}{2}$ °.		
	2	49 4	6 48		56			
	3	50 14	4 5 37		55,5			
			Mean		11 57 56-			
			Equation		-3,6			
			☉ on meridian		11 57 52,4			

II.								
h m s			h m s			h m s		
☉ sup. limb	1	7 52 32	3 20		56	☉ on the meridian. I. 11 57 52,4 II. 56 III. 55,5		
	2	53 42	2 9		55,5			
	3	54 12	1 40	11 57	56			
low. limb	1	55 23	4 0 29		56	Mean of all 11 57 56- or, true time of noon by the clock. At 4 ^h 15' thermometer 85 $\frac{1}{2}$ °.		
	2	56 34	3 59 18		56			
	3				56			
			Mean		11 57 56-			
			Equation		-3,5			
			☉ on meridian		11 57 52,5			

III.								
h m s			h m s			h m s		
☉ sup. limb	1	7 57 38	58 11		54,5	☉ on the meridian. I. 11 57 52,4 II. 52,5 III. 51,7		
	2	58 50	57 0		55			
	3	8 0 2	55 49	11 57	55,5			
low. limb	1	0 52	55 19		55,5	Mean of all 11 57 52,2 or, true time of noon by the clock.		
	2	1 42	54 8		55			
	3	2 43	3 52 57		55			
			Mean		11 57 55,1			
			Equation		-3,4			
			☉ on meridian		11 57 51,7			

Observation

Observation of the TRANSIT of VENUS.

I chose to observe the transit with the 2 feet telescope, as I supposed most of the observations in other parts would be made with telescopes of that size; and I used a power that magnified 90 times, which gave a very distinct view of the spots then on the Sun. Soon after two o'clock, I began to look on the Sun's upper limb, where the Planet was to enter. The first impression I perceived was at $2^h 27' 51''$, by the clock, the Sun being then perfectly clear. I then rested my eye, which was pretty much fatigued, to prepare it for the total ingress or interior contact. At $2^h 45' 15''$, I began to be doubtful whether the internal contact was not formed; but at $20''$ was satisfied that it was past, the Sun's limb being restored to its integrity, in the place where it had been interrupted by the Planet. During this interval of near $5''$, there seemed to be a duskiſhneſs in the place of contact; my idea of which is well represented by Mr. Dunn's figure of what he calls the grey contact, in Phil. Trans. Vol. LII. Tab. VII. p. 190.

By the foregoing equal altitudes it appears, that the clock was now $2' 13''$ + too slow. I therefore ſtate the obſervation as follows:

	Apparent time.
	h ' "
First viſible impreſſion of Venus upon the Sun	2 30 4
Internal contact	47 30

This time of internal contact, I think, cannot differ above $2''$ from the truth, and perhaps may not differ

differ $1''$. But about $4''$ of Venus's diameter must have entered upon the Sun before I perceived the impression. At nine in the morning, I observed the Sun's diameter, in the horizontal direction, to be $1\ 21\ 1$ parts of the micrometer, $= 31' 33'', 2$. At $5^h\ 34' 23''$, the Sun's north limb was distant from Venus's south limb $9\ 3$ of the micrometer, $= 6' 16'', 2$. At $5^h\ 37' 23''$, I found no sensible difference; and the Sun's north limb was then distant from Venus's north limb $7\ 14\frac{1}{2}$ of the micrometer, $= 5' 17'', 6$. This gives Venus's diameter $58'', 6$; and the least distance of centers $9' 59'', 7$. Hence, the true duration of the ingress should be $18' 56''$; but this being here contracted $15''$, by parallax, is reduced to $18' 41''$. So that the first contact, strictly so called, happened $1\frac{1}{4}$ before the impression was discovered; and the central ingress was at $2^h\ 38' 5''$. The nearest approach was nearly, I suppose, at $5^h\ 37'$.

After Venus was entered upon the Sun, I viewed her attentively several times, with a power of the great telescope which magnified 260 times, but could perceive no such duskyishness round her as I saw at the internal contact, nor that imperfect light upon her disk, especially near the centre, which Mr. Dunn speaks of; neither could I discover any satellite. Soon after six, the western sky began to be over-cast, so that for a considerable time before sun-set the Sun was hid.

I made observations for determining some other positions of Venus upon the Sun; but as they can be of no service in the grand problem of the parallax, I think it needless to swell this letter, very long already, with them. I therefore only add, that the latitude of
this

this place is nearly $42^{\circ} 25'$ N. and the difference of meridians well from London about $4^{\text{h}} 44'$. But this may be farther ascertained by the following emersions of Jupiter's satellites, which I observed with the 2 feet reflector.

Emerisions of Jupiter's first satellite.

Apparent time.									
		h	'	''		h	'	''	
1768	April 25	9	13	52	1769	May 14	10	19	7
	May 18	9	27	27		Aug. 23	7	31	50
	June 10	9	37	25	<hr/>				
	July 3	9	45	54	Emerision of J^{r} 's second satellite.				
					June 7 9 1 15				

I submit the foregoing observations to your candid acceptance; and am, with great respect,

REVEREND SIR,

Your most obedient humble servant,

John Winthrop.

*XLVII. Of the different Quantities of Rain,
which appear to fall, at different Heights,
over the same Spot of Ground. By
William Heberden, M. D. F. R. S.*

Read Dec. 7,
1769.

A COMPARISON having been made between the quantity of rain, which fell in two places in London, about a mile distant from one another, it was found, that the rain in one of them constantly exceeded that in the other, not only every month, but almost every time that it rained. The apparatus used in each of them was very exact, both being made by the same artist; and upon examining every probable cause, this unexpected variation did not appear to be owing to any mistake, but to the constant effect of some circumstance, which not being supposed to be of any moment, had never been attended to. The rain-gage in one of these places was fixed so high, as to rise above all the neighbouring chimnies; the other was considerably below them; and there appeared reason to believe, that the difference of the quantity of rain in these two places was owing to this difference in the placing of the vessel in which it was received. A funnel was therefore placed above the highest chimnies,

chimnies, and another upon the ground of the garden belonging to the same house, and there was found the same difference between these two, though placed so near one another, which there had been between them, when placed at similar heights in different parts of the town. After this fact was sufficiently ascertained, it was thought proper to try, whether the difference would be greater at a much greater height; and a rain-gage was therefore placed upon the square part of the roof of Westminster Abbey, being at such a distance from the western towers, as probably to be very little affected by them, and being much higher than any other neighbouring buildings. Here the quantity of rain was observed for a twelvemonth, the rain being measured at the end of every month, and care being taken, that none should evaporate, by passing a very long tube of the funnel into a bottle through a cork, to which it was exactly fitted. The tube went down very near to the bottom of the bottle, and therefore the rain, which fell into it, would soon rise above the end of the tube, so that the water was no where open to the air except for the small space of the area of the tube: and by trial it was found, that there was no sensible evaporation through the tube thus fitted up.

The following table will shew the result of these observations.

From July the 7th, 1766, to July the 7th, 1767,
there fell into a rain-gage fixed

	Below the top of a house. inch.	Upon the top of a house. inch.	Upon West- minster Ab- bey. inch.
1766 from the 7th of July to the end	3,591	3,210	2,311
August	0,558	0,479	} 0,508
September	0,421	0,344	
October	2,364	2,061	1,416
November	1,079	0,842	0,632
December	1,612	1,258	0,994
1767 January	2,071	1,455	1,035
February	2,864	2,494	1,335
March	1,807	1,303	0,587
April	1,437	1,213	0,994
May	2,432	1,745	1,142
June	1,977	1,426	} 1,145
from the 1st of July to the 7th	0,395	0,309	
	22,608	18,139	12,099

By this table it appears, that there fell below the top of a house above a fifth part more rain, than what fell in the same space above the top of the same house, and that there fell upon Westminster Abbey not much above one half of what was found to fall in the same space below the tops of the houses. This experiment has been repeated in other places with the same event. What may be the cause of this extraordinary difference has not yet been discovered; but it may be useful to give notice of it, in order to prevent that error, which would frequently be committed in comparing the rain of two places without attending to this circumstance.

It is probable, that some hitherto unknown property of electricity is concerned in this phenomenon. This

power has undoubtedly a great share in the descent of rain, which hardly ever happens, if the air and electrical apparatus be sufficiently dry, without manifest signs of electricity in the air. Hence it is, that in Lima, where there is no rain, they never have any lightning or thunder (*a*); and that, as M. Tournefort was assured, it never rains in the Levant but in winter, and that this is the only season in which any thunder is heard (*b*). If this appearance therefore could be accounted for, it would probably help us to some more satisfactory causes of the suspension of the clouds, and of the descent of rain.

(*a*) See the English translation of the voyage of Don George Juan and Don Antonio de Ulloa to South America, vol. II. book i. chap. 6. p. 69 and 79.

(*b*) Voyage du Levant, let. X. p. 429.

XLVIII. *An Account of an Observation of an Eclipse of the Moon, made at Hawkhill, near Edinburgh. In a Letter to the Astronomer Royal, from James Lind, M. D.*

Edinburgh, December 14,
1769.

DEAR SIR,

Read Dec. 21, 1769. **I** HERE send you our observations on the eclipse of the Moon, the 12th current; I wish it had rather been an account of an occultation, but the seeing of them seems to be denied to us: the night of the last was the only cloudy night that has been here for these four weeks past; the weather having been more like summer weather than that of winter.

The morning of the eclipse was very clear, and inclining to frost. Before we got to the observatory, near one third of the Moon's disc, where the first contact began, was covered with a smoaky appearance, which made us apprehend the eclipse was begun; but, on getting to the observatory, we saw, by our telescopes, the Moon's limb was still untouched;

A a a 2

about

about five minutes after, at $16^h 30' 51''$ mean time, a thick darkness came on the Moon's upper limb, which was still distinctly to be seen through it. At $16^h 39' 21''$ the limb was broke. The middle of the eclipse was reckoned to be the middle time betwixt the disappearing of this limb, and the re-appearing of the other. I likewise send you the sidereal times, by which the observations were taken, each observation being corrected to less than half a second; the mean times are not so near, being taken from the other by means of a table; but are still correct enough for observations which, of themselves, cannot be observed with great precision.

ECLIPSE OF THE MOON,

December 12, 1769, at Hawkhill.

	Sid. time.				Mean time.		
	h	'	"		h	'	"
1st contact of darkness	9	59	19	=	16	30	51
Moon's limb broke	10	7	50	=	16	39	21
Clear spot in or about Terra Pruinæ touched	10	15	22	=	16	46	51
Ditto disappeared	10	16	02	=	16	47	31
Copernicus { touched	10	28	58	=	17	0	25
{ central	10	30	32	=	17	1	59
{ disappeared	10	31	32	=	17	2	59
Mare Crisium touched	10	59	53	=	17	31	15
Ditto disappeared	11	9	23	=	17	40	44
Middle of eclipse	11	32	37	=	18	3	54
							1ft

[365]

	Sid. time.	Mean time.
	h / //	h / //
1st clear spot re-appeared, but indistinctly, being a little cloudy	12 14 24	= 18 45 34
Mare Crisium totally emerged	12 47 10	= 19 18 29
Moon's limb compleated	12 57 24	= 19 28 27
2d contact of darkness	12 59 24	= 19 30 27
End of smoaky appearance	13 5 25	= 19 38 27

Towards the end it was very clear, and I make no doubt but we should have seen the smoaky appearance, had it not been for the day-light coming on.

Since I wrote to you last, we have taken another meridian observation for the latitude, and made it $55^{\circ} 57' 30''$ N.

In looking into the Abridgment of the Philosophical Transactions, Vol. VII. p. 140, I found an account of an annular eclipse of the Sun, observed at Edinburgh, by Mr. Mac Laurin, in the year 1737; which may help to determine our longitude for the present, till it is done more correctly by occultations of the stars by the Moon, or eclipses of Jupiter's satellites. I am, with respect,

S I R,

Your most devoted, humble servant,

James Lind.

REMARKS.

REMARKS BY THE ASTRONOMER ROYAL.

The beginning of the eclipse was observed at the Royal Observatory at $10^{\text{h}} 20' 29''$, and the bisection of Copernicus at $10^{\text{h}} 43' 23''$ sidereal time; which, compared with the correspondent observations above, give $12' 39''$ and $12' 51''$ of time, for the difference of meridians of Hawkhill and Greenwich.

Nevil Maskelyne.

Received October 5, 1769.

XLIX. *An Account of Two Auroræ Boreales observed at Oxford. In Two Letters to Mathew Maty, M. D. Sec. R. S. from the Rev. John Swinton, B. D. F. R. S. Custos Archivorum of the University of Oxford, Member of the Academy degli Apatisti at Florence, and of the Etruscan Academy of Cortona in Tuscany.*

GOOD SIR,

Read Dec. 7, 1769. **C**ASTING my eye towards the northern part of the hemisphere, on Sunday, February 26, 1769, about 8^h 30' P. M. I discovered there a pretty bright *Aurora Borealis* of the common kind. For a short time, there was a very quick succession of lucid columns, and coruscations; which seemed smaller than they usually are in such meteors, that appear often enough here. They were of so pale a yellow colour, that some of them seemed to be almost perfectly white. In this, however, nothing very remarkable, or uncommon, could be discerned.

Butt

But what principally engaged my attention, at this time, was the gradual approach of the *Aurora* towards the south, infomuch that though it was at first most apparently an *Aurora Borealis*, and that of the common kind; it nevertheless, by the gradual variation of its original position, seemed to have commenced a sort of *Aurora Australis*, (1) of which uncommon species of meteors I have given a short account in one of my former papers, before its extinction. This happened a little before nine o'clock; after which nothing worthy of notice, as far as I could learn, during the remainder of the night, in any part of the heavens, occurred.

As the gradual variation of the original position, or situation, of this phænomenon seemed to me a pretty extraordinary circumstance, and was such a one as I had never observed, nor heard of, before; I thought myself hereby sufficiently authorized to communicate the very concise description of this meteor, now sent you, to the Royal Society. You will therefore be so good as to excuse the trouble given you by the communication; and believe me to be, with all possible consideration and esteem,

S I R,

Your most obedient humble servant,

Christ-Church, Oxon.

Sept. 28, 1769.

John Swinton.

(1) *Philosoph. Transact.* vol. LIV. for the year 1764, p. 328, 329. Lond. 1765.

L E T T E R

Received October 5, 1769.

LETTER II.

GOOD SIR,

Read Dec. 7, 1769. **B**EING in my parlour, with the
 fashes down, on Saturday, September 9, 1769, at 8^h 20' P.M. I observed, with no small degree of astonishment, through the glass, such a redness in the sky as proceeds from the reflection of a great fire. This I was at first inclined to consider as a sort of deception, occasioned by the glass through which so uncommon an object seemed to present itself to my view; but stepping out immediately into the yard, I found it to be a real appearance, resembling a flame, in the atmosphere, and consequently a very unusual sight. The meteor was, however, of no very considerable extent; being almost intirely confined to that small tract of the heavens occupied by *Ursa Major*, part of *Ursa Minor*, and the intermediate space, containing the tail of *Draco*, between those two constellations. It remained about 20', after I first discovered it, without any material change, or variation; and at 8^h 40' P.M. as I found by consulting my watch, almost instantly disappeared.

The wind on the 9th was, for the most part, W. and S. W. and the weather showery. The rain,

however, notwithstanding the favourable situation of the wind, was somewhat cold, and the whole day had a lowering winterly aspect. A small shower fell, just before I discovered the phenomenon here described. The light cast by it was nearly equal to that of the full moon, on a cloudy night. The 10th the wind continued in the same quarter as before; and the weather was much the same, attended by a disagreeable chillness in the air, as that of the preceding day. All the principal stars of the above-mentioned constellations very clearly and distinctly appeared, through the seemingly fiery vapour, with which the tract occupied by them was so strangely and so remarkably tinged.

As the luminous appearance seen at London, between eight and nine o'clock, the same night, from the short account given of it in one of the public (1) papers, seems to have agreed in all respects with that observed by me at Oxford, at the very same time, it may be considered, without any impropriety, as the very meteor here described. Admit this, and I can see nothing improbable or unnatural in such a supposition, and it must be allowed, that the atmosphere was at London in the same disposition, with regard to the exhibition of this species of meteors, as at Oxford, the very same instant of time; and impregnated in both places with the same kind of luminous vapour, at that instant, which occasioned the production of the phenomenon I have here been endeavouring to describe.

(1) *The Gazetteer and Evening Advertiser*. N^o 1265. SEPTEMBER 11, 1769.

It may not be improper to observe, that the luminous appearance of September 9, 1769, in several respects, was similar to that most remarkable one seen by me here, December 5, 1737; but differed from it in the three following particulars: 1. The former of these was not so red, nor did so much resemble the colour of blood, as the latter. 2. The former did not tinge near so considerable a part of the hemisphere as the latter. 3. The meteor of December 5, 1737, which I perfectly well remember, was of a much longer duration, than that of September 9. 1769. All which will very clearly appear from Dr. Huxham's short description of the former of these meteors, transmitted to the Royal Society, and printed in the (2) *Philosophical Transactions*, compared with the short account of the latter, drawn up by me, in the preceding part of this paper. The meteor of December 5, 1737, different from most, if not all the others, that had till that time been observed, was looked upon, if I remember right, as a singular appearance, by the great Dr. Halley himself. For, that most excellent astronomer, mathematician, and physiologist, I think, told me, when we dined together, at Mr. Swete's, in Greenwich, the following year, that he had never met with a similar phenomenon, in the whole course of his observations.

I must not forget to relate, that a most transcendent brightness, or very uncommon illustration of the atmosphere in the north, presented itself to my view, on September 9, 1769, at 10^h 15' P.M. which

(2) *Philosoph. Transact.* vol. XL. for the month of *December*, 1738, p. 437, 438.

covered about half of the interjacent space between *Ursa Major* and the horizon. The light cast by this most remarkable *crepusculum* seemed much to exceed that of the full moon. It was not, however, of any long continuance; the whole being absorbed by dark fuscous clouds, in less than a quarter of an hour after I first perceived it. This I could not help considering as a sort of sequel to the unusual phænomenon that had presented itself to my view about two hours before.

Since I began this paper, I have been informed by Mr. Parsons, student of Christ-Church, that he and the Reverend Mr. Whitchurch, likewise student of Christ-Church, on Thursday, September 21, 1769, between 8^h and 9^h P. M. discovered a dark and blackish cloud, or vapour, in the north, contiguous to the horizon; from whence issued a meteor, which bore a much greater resemblance to that of December 5, 1737, both in colour and extent, than the luminous appearance I had observed twelve days before. The colour of this meteor was so vivid, and the whole formed so lively a representation of a great deep red flame, that those gentlemen, as well as others that saw it, imagined some of the houses in the town to be set on fire; but, upon inquiry, found that no such accident had happened. It tinged a considerable tract in the northern part of the hemisphere, and particularly the space between *Ursa Major* and the horizon. It soon grew very pale, and its total extinction was so fully completed by 8^h 45' P. M. that not the faintest traces of it could then be discerned.

As very few, if any, instances of this species of meteors, which greatly differ from the common *Auroræ Boreales*, have occurred since Dr. Halley's death, which happened in the beginning of 1742; and as I am fully persuaded of his having declared to me at Greenwich, as has been already observed, that he had met with only one of them, in the whole course of his observations; I was inclined to believe, that the short account of another, or rather two others, of them, transmitted to you in this paper, would not prove unacceptable to the Royal Society, and therefore that you would excuse the trouble given on this occasion, by,

GOOD SIR,

Your much obliged,

and most obedient,

humble servant,

Christ-Church, Oxon.

Sept. 30, 1769.

John Swinton.

P. S. October 2, 1769. We had last night here some beautiful *Auroræ Boreales*, of the common kind; which began to appear in the north and north-east, a little after 8^h, and continued till near 9^h P. M. The flashes, or coruscations, were of a very pale yellow colour, and some of them ascended up to the zenith; the undulations of shining matter were quick and numerous, and the city almost as light as if illuminations had been made in several parts of it.

L. Obfer-

L. *Observations of the Transit of Venus on June 3, 1769, and the Eclipse of the Sun on the following Day, made at Paris, and other Places. Extracted from Letters addressed from M. De la Lande, of the Royal Academy of Sciences at Paris, and F. R. S. to the Astronomer Royal; and from a Letter addressed from M. Messier to Mr. Magalhaens.*

Read Dec. 14,
1769.

M. MESSIER, with the best achromatic telescope at Paris, of 12 feet focus, made by M. Anthaulme, observed the first internal contact at $7^h 38' 43''$, apparent time reduced to the Royal Observatory, and, he thinks, without an uncertainty of two seconds: and this is the observation in which I most confide. M. Du Sejour, and M. Cassini, the son, at the Royal Observatory, with much less telescopes, observed it also at $7^h 38' 43''$. M. de Fouchy, M. Bailly, M. De Borry, and two opticians, who were at the *Meute*, observed the contact at $7^h 38' 45''$, reduced to Paris. M. Cassini de Thury, at the Royal Observatory, noted it at $7^h 38' 53''$; M. the Duke de Chaulnes at $7^h 38' 57''$; both with new achromatic telescopes of Dollond of $3\frac{1}{2}$ feet. M. Maraldi, at the Royal Ob-
servatory,

servatory, observed at $7^h 38' 50''$, with a good achromatic telescope of 3 feet, made at Paris, but he thinks the observation liable to an error of ten seconds. M. Le Mannier, at St. Hubert, observed at $7^h 38' 51''$, reduced to Paris (by adding $1' 58''$). M. Fouguere, at Bourdeaux, at $7^h 38' 50''\frac{1}{2}$, reduced to Paris, taking in the difference of parallax, which is two seconds greater at Paris than Bourdeaux.

We have also received several observations of the eclipse of the Sun. M. de Thury saw the beginning at $6^h 46' 49''$ apparent time. M. Jeurat, at $6^h 46' 40''$, at the Military School, which is $7''\frac{1}{2}$ to the west of the Observatory. M. Maraldi saw the end at $8^h 27' 11''$. M. Jeurat at $8^h 27' 4''$, or $8^h 27' 11''\frac{1}{2}$ reduced to the Observatory. M. Meffier at $8^h 27' 24''$.

The observers at Rochfort, Lyons, and Avignon, did not see the transit of Venus; it was observed at Brest at $7^h 12' 5''$, or forty seconds later than at Paris, if we suppose the difference of the meridians to be well known. This point we shall examine hereafter. For my part, I could not observe the internal contact of Venus; I was precisely in the place where the clouds came on twenty-five seconds too soon; neither was it observed at the Military School, which is close to Paris.

There was also another observer at Brest, M. Verdun, an officer of the marine, who observed the internal contact of Venus at $7^h 11' 37''$, apparent time, which makes $7^h 38' 58''$, reduced to Paris. The end of the Solar eclipse was observed at Brest at $7^h 56' 33''$ and $7^h 56' 44''$ by the different observers.

M. Pingré,

M. Pingré, at Cape Francois, observed the two contacts of Venus in the latitude of $19^{\circ} 47'$ at $2^h 26' 12''$, and $2^h 44' 44''$, apparent time, with a five feet achromatic telescope, but we do not yet know the longitude of the place sufficiently. I expect him to return soon, and that he will himself draw the conclusions from his observations. At Martinico, one of our missionaries observed the contacts at $3^h 15' 14''$, and $3^h 33' 57''$; when we have the longitude exact, this observation will also be of use.

I will send you shortly all the observations of the 1st satellite, which I can collect. Here are some made at Gottingen, which I have just received from Mr. Liunberg.

1769 April 21	Im. I Sat.	13	21	37	Refractor of 14 feet.
28		15	14	34	Ditto of 10 f. of Liberkuhn.
30		9	43	21	The same.
May 23	Em. I	12	6	31	

So far M. De Lalande.

The Extract of M. Messier's Letter to M. Magalhães is as follows:

I observed the transit of Venus, June 3, 1769, at the College of Louis le Grand at Paris, which is $2''$ to the east of the meridian of the Royal Observatory. I had an achromatic telescope, of 12 feet focus, which had an aperture of $3\frac{3}{4}$ inches, and magnified 180 times, with the view of making my observation more correspondent to that which M. the Abbé, Chappe was to make, in California, with a telescope of the same length, the same magnifying power, and equal goodness. The first contact could not be seen, on account of a very thick cloud; there even fell some rain.

rain. I waited for the second; the Sun then was pretty clear. But there were some vapours, which caused such great undulations as to hinder me from seeing the disc of the Sun, and that of Venus, well defined. At $7^h 38' 45''$, apparent time, or $7^h 38' 43''$, reduced to the meridian of the Royal Observatory, the second contact appeared decisively to me. Two seconds after, a very fine thread of light appeared between the limb of Venus and that of the Sun; so that in my observation there is not an uncertainty of two seconds in the moment of the internal contact. After this observation, I viewed the Sun with different glasses, which rendered him alternately red and white. I saw Venus, with this last colour, with a crescent of a blueish colour; and a little inclined towards the limb of the Sun: with the glass which made the image of the Sun red this crescent disappeared; but I saw Venus flattened in the direction of the crescent. I measured the greatest and least diameter; the greatest was $56''\frac{1}{2}$, and the least $53''\frac{1}{2}$. Perhaps this crescent was only visible by the effect of some optic illusion; but I relate only what I saw. At $7^h 52' 8''$, apparent time, I measured the interval between the limb of Venus and that of the Sun, which I found $46''\frac{1}{2}$; and at $7^h 58' 4''$, the first limb of Venus touched the horizon.

By a letter from M. l'Abbé Bourriot to Mr. Magalhães it appears, that Mess. de Fouchy and Bailly, at the Meute, each made use of reflecting telescopes of 30 inches focus and $4\frac{1}{2}$ inches aperture; that M. Bory made use of an achromatic telescope of 5 feet focus, and 2 inches aperture; and M. l'Abbé Bourriot made use of a very good achromatic telescope of 6 feet long, and $2\frac{1}{4}$ inches aperture, made by himself, magnifying 120 times.

Read December 14, 1769.

LI. *Transit of Venus over the Sun, observed June 3, 1769, by Alexander Aubert, in Austin Friars, London, three Seconds of Time East of St. Paul's, with a Cassegrain Reflector of J. Short, having a Metal of two Feet focal Length, and magnifying about 110 Times.*

	h	'	"	
External contact at	7	8	13	mean time.
Internal contact at	7	26	45	interval 18' 32".

N.B. At 7^h 26' 45" Venus appeared to me in contact with the Sun, and about 6" after I saw the Sun's limb compleated.

The clock could be depended on to less than one second, having been compared with a number of equal altitudes of the Sun, some days before and after the transit.

Alexander Aubert.

Received October 31, 1769.

LII. *Some Account of an Oil, transmitted by Mr. George Brownrigg, of North Carolina. By William Watson, M. D. R. S. S.*

To the ROYAL SOCIETY.

GENTLEMEN,

Read Dec. 14, 1769. **T**HE application of natural productions to the benefit of mankind, has always been an object of our excellent institution; and endeavours to extend the utility of substances already very obscurely known, have always met from you a favourable reception.

It is with this view, that I lay before you some pods of a vegetable, and the oil pressed from their contents. They were sent from Edenton, in North Carolina, by Mr. George Brownrigg, whose brother, Dr. Brownrigg, is a worthy member of our Society; and are the produce of a plant well known, and much cultivated, in the Southern colonies, and in our American sugar islands, where they are called ground

nuts, or ground pease. They are originally, it is presumed, of the growth of Africa, and brought from thence by the negroes, who use them as food, both raw and roasted, and are very fond of them. They are therefore cultivated by them in the little parcels of land set apart for their use by their masters. By these means, this plant has extended itself, not only to our warmer American settlements, but it is cultivated in Surinam, Brasil, and Peru.

The plant, which produces these, has been mentioned, and described, by the botanical writers of the later times. Ray, in his History of Plants, calls it *Arachis Hypogaios Americanus*. It is the *Arachidna quadrifolia villosa* of Plumier. Sir Hans Sloane, in his History of Jamaica, calls it *Arachidna Indica utriusque tetraphylla*. Piso and Marcgraac both mention it among the Brasilean plants, under the name of *Mundubi*. Linnæus has constituted a genus of this plant, of which only one species is as yet known, under Mr. Ray's generical name of *Arachis*.

This plant, together with a very few of the trifoliate tribe, has the property of burying its seeds under ground, which it does in the following manner. As soon as the plant is in flower, its flower is bent towards the ground until it touches it. The pointal of the flower is then thrust into the ground to a sufficient depth, where it extends itself, and forms the seed-vessel and fruit, which is brought to maturity under ground, from whence it is dug up for use.

This plant, which is a native of warm climates, will not bear being cultivated to advantage in Great-Britain, or in the northern colonies; but, according to Mr. Brownrigg, in southern climates its produce

is prodigious ; and what adds to its value is, that rich land is not necessary for its cultivation, as light sandy land, of small value, will produce vast crops of it. Besides what the negroes cultivate for their own use, some planters raise a considerable quantity of it, for the feeding of swine and poultry, which are very fond of the ground pease ; and, when they are permitted to eat freely of them, soon become fat.

Mr. Brownrigg, from whom, as I before mentioned, I received the oil, considers the expressing oil from the ground pease, as a discovery of his own : it may, perhaps, at this time, be very little practised either in North Carolina, the place of his residence, or elsewhere. But certain it is, that this oil was expressed above fourscore years ago ; as Sir Hans Sloane mentions it, in the first volume of his History of Jamaica ; and says, that this oil is as good as that of almonds. It is probable, however, that small quantities only were expressed, and that even at that time the knowledge of it did not extend very far. Mr. Brownrigg therefore is highly praise-worthy in reviving the remembrance of procuring oil from these seeds. It is obtained, by first bruising the seeds very well, and afterwards pressing them in canvas bags, as is usual in procuring oil from almonds or linseed.

To have the oil in the best manner, no heat should be used. The heating the cheeks of the press increases the quantity of the oil, but lessens its goodness, where it may be intended to be used as food, or as a medicine. For other purposes, the larger quantity of oil, obtained by heat, will answer equally well.

Neither the seeds nor oil are apt to become rancid by keeping ; and as a proof of this, the oil before you,

which was sent from Carolina in April last; and, without any particular care, has undergone the heats of last summer, is yet perfectly sweet and good. These seeds furnish a pure, clear, well tasted oil; and, as far as appears to me, may be used for the same purposes, both in food and physic, as the oils of olives or almonds. It may be applied likewise to many, if not all, the economical purposes with the former of these.

But what greatly adds to the merit of what Mr. Brownrigg has informed us of, is the low price, at which this oil may be obtained. He says, that ten gallons of the pease, with the husks unshelled, will, without heat, yield one gallon of oil; if pressed with heat, they will afford a much larger quantity. The value of a bushel of these, in Carolina, does not exceed, as I have been informed, eight pence, or thereabouts. These will furnish a gallon of oil, the labour and apparatus to procure which, cannot cost much. This price will not amount to so much as a fourth of what the best Florence oil of olives costs in England. This therefore ought to be considered as valuable information, as, on account of its cheapness, a larger portion of mankind than at present may be permitted to use oil with their food, from whom it is now withheld on account of its price.

Great quantities of olive oil are sent from Europe to America. New England alone, Mr. Brownrigg says, annually consumes twenty thousand gallons. The quantities used in his majesty's other dominions in America must be prodigious. The oil from ground pease, of which any quantity desired may be raised, may and would supply this consumption of olive oil. It would likewise, I am persuaded, bear exportation

to any of those places where the oil of olives is usually carried; and thereby become a valuable article of commerce.

After the oil has been expressed from the ground pease, they are yet excellent food for swine.

Presuming that a more intimate knowledge of the vegetable production before you, than what we were lately possessed of, would not be disagreeable to the Royal Society, I take the liberty of laying the present account before you; and am,

GENTLEMEN,

Your most obedient,

humble servant,

William Watson.

LIII. *A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society by the worshipful Company of Apothecaries, for the Year 1768, pursuant to the Direction of Sir Hans Sloane, Bart. Med. Reg. et Soc. Reg. nuper Præses. By William Hudson, Societatis Regiæ et clariss. Societatis Pharmaceut. Lond. Soc. Hort. Chelsean. Præfectus et Prælector Botanicus.*

Read Dec. 14. 2301
1769.

ACONITUM *uncinatum*,
foliis multilobis, corollarum
galea apice basi longius producto. Lin.
Spec. plant. 750.

2302 *Adiantum Capillus-Veneris*, frondibus decompositis, foliolis alternis, pinnis cuneiformibus, lobatis pedicellatis. Lin. Spec. plant. 1558. Hudf. fl. Angl. 391.

Adiantum foliis coriandri. Bauh. pinn. 356.

2303 *Andropogon, Ischaemum*, spicis digitatis plurimis, flosculis sessilibus, aristato muticoque, pedicellis lanatis. Lin. Spec. plant. 1483.
Gramen dactylon spicis villosis. Bauh. pin. 8.

2304 An-

- 2304 *Anthemis maritima*, foliis pinnatis dentatis
carnosis nudis punctatis, caule prostrato, caly-
cibus tomentosis. Lin. Spec. plant.
1259.
Matricaria maritima. Bauh. pin. 134.
- 2305 *Anthemis mixta*, foliis simplicibus dentato-laciniatis. Lin. Spec. plant. 1260.
Chamæmelum annuum ramosum coronopifolio, flore mixto. Hist. Ox. III. 36. tab. 18. fig. 15.
- 2306 *Anthyllis coccinea*, herbacea, foliis pinnatis inæqualibus, capitulo duplicato. Lin. Spec. plant. 1012. Hudf. fl. Angl. 273.
Vulneraria supina, flore coccineo. Dill. Hort. Elth. 431. tab. 320. fig. 413.
- 2307 *Asclepias curassavica*, foliis lanceolatis petiolatis glabris, caule simplici, umbellis erectis solitariis. Lin. Spec. plant. 314.
Apocynum radice fibrosa, petalis coccineis, corniculis croceis. Dill. Hort. Elth. 34. tab. 30. fig. 33.
- 2308 *Ascyrum hypericoides*, foliis oblongis, ramis ancipitibus. Lin. Spec. plant.
Hypericum pumilum semper virens, caule compresso ligneo, ad bina latera alato, flore luteo tetrapetalo. Pluk. plant. 104. R. Hist. III. 495.
- 2309 *Asplenium, ruta muraria*, frondibus alternatim decompositis, foliolis cuneiformibus crenulatis. Lin. Spec. plant. 1541.
- 2310 *Ballota alba*, foliis cordatis indivisis serratis, calycibus subtruncatis. Lin. Spec. plant. 814.
- Vol. LIX. D d d Ballote

- Ballote flore albo. Tourn. Inst. 185.
- 2311 *Bidens bullata*, foliis ovatis serratis, inferioribus oppositis, superioribus ternatis, intermedio majore. Lin. Spec. plant. 1167.
- 2312 *Cassia filiformis*. Ait. it. 243. Lin. Spec. plant. 530.
- Cuscuta baccifera* Barbadenfium. Pluk. phyt. tab. 172. f. 2.
- 2313 *Cenchrus racemosus*, panicula spicata, glumis muricatis, fetis ciliaribus. Lin. Spec. plant. 1487.
- Gramen caninum maritimum asperum*. Bauh. prodr. 2. tab. 2.
- 2314 *Chrysanthemum corymbosum*, foliis pinnatis, inciso-serratis, caule multifloro. Roy Lugdb. 174. Lin. Spec. plant. 1251.
- Tanacetum montanum inodorum, minore flore*, Bauh. pin. 132.
- 2315 *Cistus salicifolius*, herbaceus, patulus, villosus, stipulatus, floribus racemosis erectis, pedicellis horizontalibus. Lin. Spec. plant. 742.
- Cistus folio salicis*. Bauh. pin. 464.
- 2316 *Elymus virginicus*, spica erecta spiculis trifloris, involucrio striato. Lin. Syst. Nat. 101. Spec. plant. 123.
- 2317 *Elymus hyssrix*, spica erecta spiculis involucrio destitutis patentibus. Lin. Spec. plant. 124.
- 2318 *Erica cinerea*, antheris bicornis inclusis, corollis ovatis racemosis, foliis ternis glabris linearibus. Lin. Spec. plant. 501. Hudf. Angl. 144.
- Erica*

Erica humilis, cortice cinereo, arbuti flore.
Bauh. pin. 486.

2319 *Erigeron graveolens*, ramis lateralibus multifloris, foliis lanceolatis integerrimis, calycibus squarrosis. Lin. Spec. plant. 1219.

2320 *Fumaria capreolata*, pericarpis monospermis racemosis, foliis scandentibus subcerrhosis. Lin. Spec. plant. 985.

Fumaria major scandens flore pallidiore. Raj. Syn. 204.

2321 *Heliotropium Indicum*, foliis cordato-ovatis acutis scabriusculis, specieis solitariis, fructibus bifidis. Lin. Spec. plant. 187.

Heliotropium Americanum cæruleum. Pluk. phyt. tab. 245. f. 4.

2322 *Hordeum vulgare*, flosculis omnibus hermaphroditis, aristatis; ordinibus duobus erectioribus. Lin. Spec. plant. 125.

Hordeum polystichon vernum. Bauh. pin. 22. Hist. Ox. III. f. 8. tab. 6. f. 3.

2323 *Hordeum cæleste*, flosculis omnibus hermaphroditis feminibus decorticatis. Lin. Spec. plant. 125.

2324 *Hordeum distichon*, flosculis lateralibus masculis muticis, feminibus angularibus imbricatis. Lin. Spec. plant. 125.

Hordeum distichon. Bauh. pin. 22. Hist. Ox. III. f. 8. tab. 6. fig. 1.

2325 *Horminum virginicum*, foliis cuneiformi-oblongis, caule bifolio. Lin. Spec. plant. 832.

Melissa atrorubens bugulæ folio. Dill. Hort. Elth. 219. tab. 175. fig. 216.

- 2326 *Justicia byssopifolia*, fruticosa, foliis lanceolatis integerrimis, pedunculis trifloris ancipitibus, bracteis calyce brevioribus. Lin. Spec. plant. 21.
- 2327 *Lactuca scariola*, foliis verticalibus carina aculeatis. Lin. Spec. plant. 1119.
Lactuca sylvestris costa spinosa. Bauh. pin. 123.
- 2328 *Lathyrus, Aphaca*, pedunculis unifloris, cirrhis aphyllis, stipulis sagitato-cordatis. Lin. Spec. plant. 1029.
Aphaca. Lob. ic. 2. p. 70.
- 2329 *Lythrum, Hyssopifolia*, foliis alternis linearibus, floribus hexandris. Lin. Spec. plant. 642.
Hyssopifolia. Bauh. pin. 218.
- 2330 *Milium paradoxum*, floribus paniculatis aristatis. Lin. Spec. plant. 90.
Gramen avenaceum paniculatum galloprovinciale, aquilegiæ semine. Hist. Ox. 111.
 214. Pluk. phyt. tab. 32. f. 2.
- 2331 *Osmunda crispa*, frondibus supra decompositis, pinnis alternis subrotundis incisfis. Lin. Spec. plant. 1522. Hudf. Angl. 383.
Adiantum crispum alpinum. Raj. Syn. 126.
- 2332 *Panicum Italicum*, spica composita, spiculis glomeratis fetis immixtis, pedunculis hirsutis. Lin. Spec. plant. 83.
Panicum Italicum f. panicula majore. Bauh. pin. 27.
- 2333 *Panicum glaucum*, spica tereti involucellis bifloris fasciculato-fetosis, seminibus undulato-nervosis. Lin. Spec. plant. 83.
Panicum spica simplici, aristis aggregatis flosculo subjectis. Gron. virg. 134.
- 2334 *Passerina*

- 2334 *Passerina hirsuta*, foliis carnosis extus glabris, caulibus tomentosis. Lin. Spec. plant. 513.
Thymelæa tomentosa, foliis fedi minoris. Bauh. pin. 461.
- 2335 *Potentilla Norvegica*, foliis ternatis, caule dichotomo, pedunculis axillaribus. Lin. Spec. plant. 715.
Quinquefolium hirsutum luteum paucioribus laciniis. Loefel. Pruss. 218. tab. 70.
- 2336 *Prunella grandiflora*, foliis omnibus ovato-oblongis petiolatis. Lin. Spec. plant. 837.
Prunella cærulea, magno flore. Bauh. pin. 261.
- 2337 *Pforalea corylifolio*, foliis simplicibus ovatis. Lin. Spec. plant. 1075.
Loto affinis coryli folio. Pluk. phyt. tab. 96. f. 5.
- 2338 *Rubia peregrina*, foliis quaternis. Lin. Spec. plant. 159.
- 2339 *Salvia Hispanica*, foliis ovatis, petiolis utrinque mucronatis, spicis imbricatis, calycibus trifidis. Lin. Spec. plant. 37.
Sclarea Hispanica. Tabern. Hist. 764. ic. 374.
- 2340 *Sanicula Marilandica*, flosculis masculis pedunculatis, hermaphroditis sessilibus. Lin. Spec. plant. 339.
- 2341 *Satureia hortensis*, pedunculis bifloris. Lin. Spec. plant. 795.
Satureia hortensis. Bauh. pin. 218.
- 2342 *Saxifraga stellaris*, foliis serratis, caule nudo ramoso, petalis acuminatis. Lin. Spec. plant. 572. Hudf. Angl. 156.
- 2343 *Saxifraga*

- 2343 *Saxifraga aizoides*, foliis caulinis lineari-subulatis sparsis nudis inermibus, caulibus decumbentibus. Lin. Spec. plant. 576.
Sedum Alpinum, flore pallido. Bauh. pin. 284. Hist. Ox. III. 477. f. 12. tab. 6. f. 3.
- 2344 *Senecio hieracifolius*, corollis nudis, foliis amplexi caulibus laceris, caule herbaceo erecto. Lin. Spec. plant. 1215.
Senecio Americanus altissimus blattariæ f. *hieracii* folio. Hern. par. 226. tab. 226.
- 2345 *Serapias latifolia*, bulbis fibrosis, nectarii labio crenato, obtuso, petalis æquali, fructibus pendulis. Hudf. Angl. 341.
Serapias bulbis fibrosis, foliis ovatis amplexi-caulibus, floribus pendulis. Lin. Syst. Nat. vol. II. 593.
Helleborine altera atro-rubente flore. Raj. Syn. 383.
- 2346 *Serapias palustris*, bulbis fibrosis, nectarii labio obtuso crenato, petalis æquali, fructibus pendulis. Hudf. Angl. 341.
Serapias bulbis fibrosis, foliis ensiformibus, floribus pendulis. Lin. Syst. Nat. vol. II. 593.
Helleborine palustris nostras. Raj. Syn. 283.
- 2347 *Serratulo scarrofa*, foliis lanceolatis integerrimis, calycibus squarrosis pedunculatis obtusis lateralibus. Lin. Spec. plant. 1147.
Eupatoria affinis Americana bulbosa, floribus scariosis capitulis contextis. Pluk. phyt. tab. 177. f. 4.

- 2348 *Sideritis scordioides*, foliis lanceolatis subdentatis supra glabris, bracteis ovatis dentato-spinosis calycibus æqualibus. Lin. Spec. plant. 803.
- 2349 *Tanacetum annuum*, foliis bipinnatifidis linearibus acutis corymbis tomentosis. Lin. Spec. plant. 1184.
Absinthium corymbiferum annuum. Tourn. 458.
- 2350 *Viburnum dentatum*, foliis ovatis dentato-ferratis plicatis. Lin. Spec. plant. 384.

LIV. *A Description of the Lymphatics of the Urethra and Neck of the Bladder. By Henry Watson, Surgeon to the Westminster Hospital, and F. R. S.*

Read Dec. 14,
1769.

THE valvular lymphatics, as a system of vessels, *sui generis*, are allowed to have a very considerable office in the animal œconomy ; but an office, subordinate to that of the blood-vessels : at least, they have been supposed by many of the physiologists, not of so much consequence, in preserving the health and life of the animal.

If we consider, that an obstructed *thoracic duct*, which is in fact but a large lymphatic, will destroy life as effectually as a ligature made upon the *aorta* itself ; we must conclude the lymphatics to be vessels of much greater importance than some have imagined : nearly of as much consequence, in supporting and carrying on the animal functions, as the arteries and veins themselves : for if an obstruction of the *aorta*, or great artery, can produce a very quick, or sudden, death ; an *obstructed thoracic duct* will as certainly lead to a tedious and lingering one.

The case of the obstructed *duct*, though not indeed often seen, yet is every now and then to be met with. It is the one cause of a *marasmus* not known, or not attended to : generally owing to an enlargement of the

the lymphatic glands that lie near to, and in contact with the duct: generally too attended with obstructions in the more external conglobate glands; therefore always to be suspected, where we have these appearances, accompanied with a gradual wasting of the solids. In children and young subjects we meet with proofs of this disease; a disease, which never could have been learnt, but from the dissection of morbid bodies.

The lymphatics are said to be the true, and only system of absorbing vessels. I will suppose they are; though perhaps this opinion may yet admit of some doubts: however, they certainly are the vessels that take up the watery *latex* from most parts of the body, and return it back to be again mixed with the blood. This free absorption of the lymph is the great security against suffocation, injurious pressure, and an obstructed circulation in every part of the animal.

Many valuable discoveries have lately been made, of the existence of these vessels in birds, fish, and *amphibii*. That most accurate and indefatigable anatomist, Dr. Hunter, has writ fully and explicitly upon the lymphatics in the human body; and yet, still it is to be wished, we knew more about them.

We have not been able to see their origins in any one instance; we have not traced them through the whole body, as we have done the blood-vessels. It is reasonable to suppose they abound universally; but it is doubtful whether in many parts they exist, or not; for the most eminent anatomists confess, there are many parts, in which, hitherto, they have not been able to discover them.

It may not therefore be unentertaining to this learned Society, who so studiously promote every useful inquiry, not only to have a demonstrative proof of the existence of lymphatics in a part of the human body where they have not as yet been discovered ; but also to have an opportunity of knowing that the true origin of these vessels may easily be shewn.

As to their precise origin, it has indeed been conjectured, and very reasonably, from experiments *à posteriori*.

It has been supposed they arise from all the surfaces and cavities of the body ; because thin fluids and subtle particles will be taken up from such cavities, or surfaces, and will be readily enough conveyed into the blood : but then it has never been shewn, that they do arise from any one such surface or cavity.

Commonly, the lymphatics are never filled from their beginnings, or little orifices. When they have been injected, it has always been done by using some violence ; either by cutting into them, bursting, or tearing them asunder ; so that the injection rather gets in some how at the side, and not at the extremity of the vessel.

The lacteal vessels perhaps cannot, at least have never been, to my knowledge, injected from the cavity of the intestine in the dead body. It is presumed, that, as the lymphatics are similar to these in other respects, their origins must be also similar : that if the orifices of the lacteals are too fine to be discovered, the mouths of the lymphatics are also too delicate to be traced out. But with regard to the lymphatics of the human urinary bladder, it is certainly otherwise. When the part is fresh and sound,

we may, with a little trouble, blow into the mouths of these vessels, small as they are ; or introduce a fine bristle into them, if we have but a steady hand and a good eye. I have frequently done both, in the presence of many witnesses ; so that, without using the knife or lancet, the least force or violence, air may be thrown into the lymphatics from their very beginnings ; and mercury may be made to pass by the same orifices.

Though I have said we may easily have an ocular demonstration of the origin or mouth of the lymphatic, in this part of the human body, I must confess, it is not always we can have that satisfaction : no part is more frequently diseased : inflammation folds up the mouths of these little vessels ; and it is not to be expected we can shew their orifices when the *urethra* is in such a state.

It will always require some dexterity to catch the opening of the lymphatic ; but the bristle, once fairly introduced, will generally pass with great ease some way within the vessel.

Here then we may satisfy ourselves in what manner the lymphatics do actually begin from surfaces : and to those who, without ever having seen the origin of a lymphatic, have nevertheless reasoned so well, and so justly, upon this subject, it may perhaps afford some pleasure and satisfaction to find their conjectures agreeing so perfectly with the structure.

The situation of the lymphatics, in general, is superficial ; that is to say, they are mostly to be seen upon surfaces ; though there are some deeper seated ones, which accompany the blood vessels. They have been

well described by authors, as exceeding fine, tender, and transparent vessels, frequently joining into one another, and intersected by a number of very delicate membranous pouches or valves; so that, having an injection thrown into them, they give the appearance of being full of little knots.

The lymphatics are apparent enough, when they unite and grow large; but from their exility, want of colour, and transparency, are very difficult to be discovered before.

Owing to these circumstances it is, that their origins have never before been seen; and that in many parts of the body, where they are nevertheless supposed to exist, they still lie unnoticed. Haller, after speaking of these vessels in many other parts of the body, goes on thus: "*Quæ a pene veniunt mihi minus nota sunt, sed dicta Cowpero. Alia huc a vesiculis seminalibus tendunt, aut certe ab earum vicinia, aut a vesicæ urinariæ sede, aut ab ipsa demum vesica, quæ quidem vascula iterum fateor mihi nondum visa esse.*" So that Haller, who knows so well the structure of the human body, knows nothing of these lymphatics of the bladder, or membranous portion of the *urethra*.

The lymphatics of the urinary human bladder and *urethra*, first shew themselves on each side the *verumontanum* or *caput gallinaginis*, and by very little orifices take their origin from the internal membrane that lines the *urethra* and bladder, on whose surface they open.

In their natural state, they appear like so many fine threads lying close together, but diverging afterwards,

as they pass over the prostate gland and neck of the bladder, and inosculating or communicating very frequently, they form a kind of network or embroidery. From hence they are continued through the cellular membrane behind the internal coat of the bladder, and seem to join with the lymphatics of the *vesiculæ seminales*, to be continued with them to the neighbouring glands, and so on to the thoracic duct.

I have not been able to find lymphatics in any other part of the *urethra*; indeed, this canal seems to be perfectly void of them till we come to its membranous portion, where we meet with these I have been describing; and it may be remarked, that here they are placed in that part of the *urethra* where the greatest quantity of moisture is supplied. Very probably the sealing up the mouths of these delicate vessels, by frequent inflammation and induration, may give rise to that obstinate *stillicidium* which is seldom or ever cured; owing to an accumulation of thin fluids, with a faulty absorption.

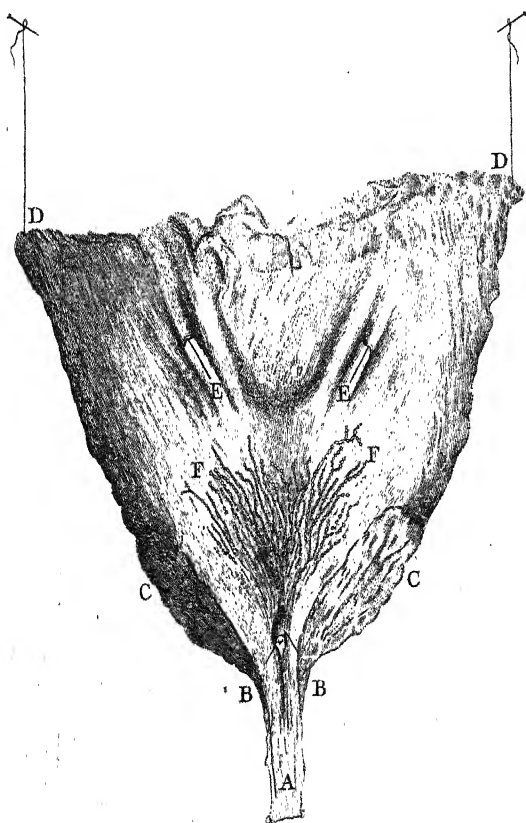
These lymphatics of the *urethra* and bladder also point out the road, by which any subtle *virus* may pass, with the lymph, directly into the mass of blood, and contaminate the whole habit, without giving the least appearance of any local disorder.

To have a clearer idea of the vessels I have been treating of, I must beg leave to refer to the drawing annexed, in which these lymphatics of the urinary bladder and *urethra*, in the human body, are carefully and very accurately delineated.

Explanation of T A B. XVI.

This drawing is an exact representation of the lymphatics of the *urethra* and neck of the bladder, as they appear after having been injected with mercury, and preserved in spirits.

- A. The membranous portion of the *urethra* slit open.
- B B. Bristles in the ducts from the *vesiculae seminales*.
- C C. Prostate gland.
- D D. The inferior part of the bladder laid open.
- E E. Bristles in the *ureters* where they open into the bladder.
- F F. The lymphatics.



Read December 21, 1769.

LV. *Eclipses of Jupiter's First Satellite, the Eclipse of the Moon, and Occultations of Fixed Stars by the Moon. Observed at the Royal Observatory at Greenwich, in the Year 1769. Communicated by the Astronomer Royal.*

		Apparent time.			
		h	'	"	
1769					
March	29 Im.	12	25	7	with a 2 feet reflector of Short's constr.
April	12 Im.	16	16	8	with a 2 feet reflector of Bird's constr.
April	28 Im.	14	35	17½	with a 2 feet reflector of Short's.
May	16 Em.	{	9	32 15	with a 2 feet reflector of Short's.
			9	31 35	with a 6 feet reflector of Short's.
June	8 Em.		9	40 56	with a 6 feet reflector of Short's.
June	15 Em.	11	35	33	with a 2 feet reflector of Short's.
					Air a little hazy.
July	1 Em.	9	50	24	with a 2 feet reflector of Short's.
					Jupiter very clear.

ECLIPSE OF THE MOON.

Observed with a 3½ feet achromatic treble object glass telescope of Dollond, with least magnifying power 30 times; and a 2 feet reflector of Short's, with least magnifying power 60 times.

		Apparent time.			
		h	'	"	
1769					
Dec. 12	16 57 13	16	57	9	Beginning of eclipse.
	17 3 3				The shadow first touches Kepler.
	17 3 33				bisects ditto.
	17 4 3	17	3	35	covers ditto.

Apparent

Apparent time.							
1769	h	'	"	h	'	"	
Dec. 12	17	9	19				The shadow touches Pytheas.
	17	10	32	17	10	30	covers ditto.
	17	13	22				covers Timochares.
	17	15	54	17	15	18	covers Archimedes.
	17	16	56				touches Eratosthenes.
	17	18	20				covers ditto.
	17	20	1				bisects Copernicus.
	17	26	1	17	26	5	touches Mare Serenitatis.
	17	30	52				touches Manilius.
	17	32	58	17	33	20	covers ditto.
	17	35	17				touches Menelaus.
	17	36	17	17	37	5	covers ditto.
	17	39	42				touches Plinius.
	17	40	17	17	40	43	covers ditto.
				17	45	14	covers Dionysius.
	17	53	9	17	53	38	covers a spot between Plinius and Promont. Somnij.
	18	6	52	18	6	55	bisects a black spot surrounded by a white circle in Terrâ Mannæ, lying in a line joining Langrenus and Kepler.

The observations in the first column were made by myself, with the $3\frac{1}{2}$ f. achromatic telescope; and the others by my assistant W. Bayley, with the 2 f. reflector.

OCCULTATIONS OF FIXED STARS BY THE MOON.

Apparent time.					
	h	'	"		
April 11	7	24	11	Em. of μ Geminorum, from the Moon's bright limb. Certain to 5 or 10 seconds, with Dollond's $3\frac{1}{2}$ feet telescope.	
May 9	8	14	49	Em. of ζ Geminorum, from the Moon's bright limb, with a 2 feet reflector. The true emersion is supposed to have been about 5 seconds sooner.	

Mean

Mean time.

		h	'	"	
Sept. 15		8	1	43,3	im. of 16 Piscium, at D's bright limb, with 6 f. reflector, certain to 1".
	between	10	22	47,3	and 49",3
					with 6 feet refl.
	between	10	22	50,3	and 51",3
					with 2 f. ref. by W. B.
Sept. 20		10	28	19,6	im. of 1st α Tauri, with both telescopes exactly the same.
		11	12	28,2	em. of 1st α Tauri, at D's dark limb, with 2 feet reflector.
		11	16	54,5	em. of 2d α Tauri, with both telescopes exactly the same.
Sept. 25		17	24	14,6	im. β Leonis, at D's bright limb, with 6 feet refl.
		17	24	12,6	ditto, with 2 f. reflector, by W. B.

The star was eclipsed to the north of the Moon's center, and the time seemed certain to less than a second, with both telescopes.

Nov. 18		14	39	3,7	im. of 2d α Cancræ, at D's bright limb, with 2 f. refl. Uncertain to 10".
		15	5	49,3	em. of ditto, at D's dark limb, instantaneous, and the same with both telescopes.

Read December 1, 1769.

LVI. *Eclipses of Jupiter's First Satellite,
with an Eighteen Inch Reflector of Mr.
Short's. Observed by Dr. Wilson at the
Glasgow Observatory.*

			Equal time.				Apparent.					
			h	'	"	'''	h	'	"	'''		
1762	Sept.	11	Im.	10	55	33	21	10	55	33	21	good.
	Oct.	4	Im.	11	1	58	—	11	13	22	21	good.
	Nov.	14	Em.	6	8	33		6	23	43	55	indifferent.
		21	Em.	8	1	31	38	8	16	39		good [ly clear.
1763	Oct.	23	Im.	12	50	24	—	13	5	56	53	good, air extreme-
	Nov.	1	Im.	9	12	18	43	9	28	29	18	good, like the for-
1764	Oct.	25	Im.	16	8	48	—	16	24	40		indif. good. [mer.
1765	Febr.	14	Em.	6	18	28		6	3	55		good.
	March	7	Em.	12	2	24		11	51	29		good.
	Nov.	29	Im.					15	53	26		indifferent.
	Dec.	22	Im.	15	54	4	—	15	54	25		indifferent.
1766	April	20		8	4	23	—	8	5	41	15	good.

Solar Eclipse.		h	'	"	h	'	"
1769	March 31 Beginning	20	58	16	20	54	15 9
	Gr. obs.	22	19	3	22	15	2
	End	23	46	55	23	42	52

1769	Nov. 6	Alt. of J's		Parts of 96.	
J's	west. limb.	Merid.	under limb.	parts.	parts.
		h ' "	° ' "	° ' "	parts.
		6 52 6	20 47 36+	22	1. 14

Ther.

Barom.	without.	This is the only one of the Moon since the quadrant was in order.
inches	°	
29.66	49 46	

N. B. The equation of time made use of, is taken from De la Caille's Ephemeris.

Observed

Observations of Eclipses of Jupiter's First Satellite, proper to be compared with the foregoing ones, in order to determine the Difference of Meridians of Greenwich and Glasgow. Communicated by the Astronomer Royal.

		App. time.					
		h ' "					
1762	Sept. 11	Im.	11	12	43	Surry-street, 2 feet reflector.	By N. Maskelyne.
	Oct. 4	Im.	11	31	1	Surry-street, 2 f. ref.	Observed by J. Short.
	Nov. 3	Em.	15	56	57½	Greenwich, 6 f. ref.	By C. Green.
	Nov. 12	Em.	12	10	56	Surry-str. 2 f. ref.	By J. Short.
1763	Oct. 16	Im.	11	27	39	Surry-str. 2 f. ref.	By J. Short.
	Nov. 1	Im.	9	45	25	Surry-str. 2 f. ref.	By J. Short.
1764	Nov. 4	Im.	13	3	37	Greenwich, 2 f. ref.	By C. Green.
	Nov. 10	Im.	14	57	11	Greenwich, 6 f. ref.	By C. Green.
1765	Feb. 19	Em.	13	46	42	Greenwich, 6 f. ref.	By C. Green, good observation.
	Dec. 1	Im.	10	40	11	Greenw. 18 inch ref.	} By N. Maskelyne
	8	Im.	12	31	34	Greenw. 6 f. ref. Air very clear.	
	15	Im.	14	22	3	Greenw. 6 f. ref.	
	22	Im.	16	12	19	Greenw. 6 f. ref.	
	24	Im.	10	39	27	Greenw. 6 f. ref.	
1766	April 11	Em.	11	56	30	Greenw. 6 f. ref.	By J. Dymond.

N. B. The late Mr. Short's house, in Surry-street, where some of the above observations were made, is $26''\frac{1}{2}$ of time west of Greenwich.

LVII. *Extract of a Letter to the Reverend Nevil Maskelyne, Astronomer Royal, from Mr. Benedict Ferner, F. R. S. Dated Stockholm, June 9, 1769. Translated from the French.*

S I R,

Read Dec. 21, 1769. **I** AM more surprized that the times of the contacts of Venus and the Sun's limbs, observed here, by different observers, with different instruments, agree so near together, than I am at their difference; for the nearness to the horizon, and the extraordinary quantity of vapours with which the atmosphere was then loaded, not only caused the limb of the Sun to tremble and undulate, but also gave it, if I may so express myself, the form of a large saw, the eminences being luminous and the cavities black, which shifted places like the waves of a tempestuous sea.

There was no reason for fixing the moment of the ingress of Venus sooner than she had made a greater cavity in the limb of the Sun than the depth of the waves or black notches; and then one might be very sure of the fact: but certainly at that time some seconds must have been passed from the beginning of the ingress. Therefore, I
am

am very well persuaded, that $8^h 24' 9''$ apparent time, which I took for the beginning of the ingress, is four, five, or seven seconds too late. I hoped to see the internal contact with more certainty; but I was mistaken; for I found as great difficulties there, though of another kind. When I judged, by means of the circular figure of the Sun's disc, that Venus should be intirely within the Sun, I could not yet see the luminous cusps of the Sun join together behind Venus, who, on the contrary, appeared to carry the limb of the Sun along with her, which appeared to bend towards Venus, leaving a black cavity in his limb; and a moment after, when I thought I saw the whole body of Venus in the Sun, a little black column appeared to proceed from Venus towards the imaginary limb of the Sun. The whole of this phenomenon was certainly, in my opinion, the effect of the tremors of the limbs of the Sun and Venus; but I took $8^h 41' 48''$ for the moment of the internal contact, when the thread of the Sun's light closed behind Venus.

The limbs of Venus were, at least, as tremulous and ill defined as those of the Sun. Sometimes Venus had black eminences, which projected so much that they were not unlike a pointed truffle. The first notch made by Venus in the Sun was not round, but resembled an obtuse angle. The diameter of Venus, which was perpendicular to the Sun's limb, appeared the greatest while Venus was passing over the Sun's limb; but after Venus had passed the Sun's limb, the same diameter appeared the smallest; so that Venus presented herself in both these cases under an oval form, but in contrary directions.

Clouds

Clouds hindered us from observing the beginning of the eclipse of the Sun ; but I observed the end of the eclipse, at $10^h 4' 53''$ apparent time, with an achromatic telescope of Dollond, of 10 feet, magnifying 96 times ; the same telescope which I used in observing the transit of Venus. The difference of meridians between Stockholm and Upsal is $1' 40''$ of time.

I have the honour to be,

with the greatest friendship and esteem,

S I R,

Your most obedient,

humble servant,

B. Ferner.

LVIII. *Observations of the Transit of Venus over the Sun, on June 3, 1769; and the Eclipse of the Sun the next Morning; made at East Dereham, in Norfolk, by the Rev. Francis Wollaston, F. R. S. Extracted from some Letters addressed to the Rev. Nevil Maskelyne, F. R. S. and Astronomer Royal.*

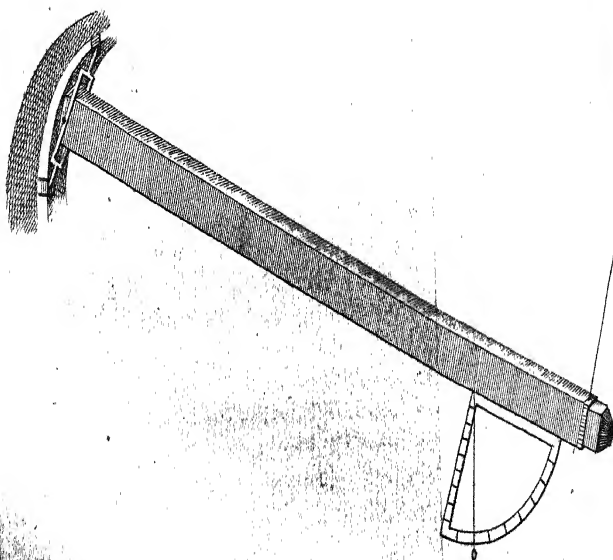
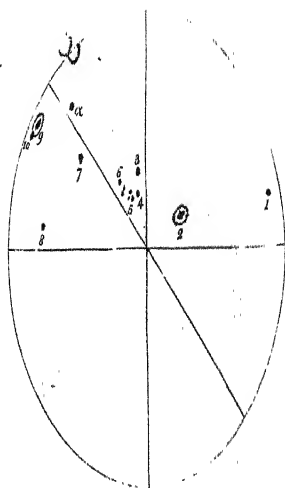
Read Dec. 21,
1769.

THE telescope I used was a reflector of Short's, of 12 inches focus, with a power that magnifies about 55 times. My clock was made by Holmes. It escapes dead seconds, has a pendulum with a wooden rod, and is firmly fastened to a stack of chimnies in the room where I observe. I had lately received it from London, and regulated it by transits of the Sun and stars, taken with a transit instrument of a peculiar kind (described at the end of this account); and also by equal altitudes of the Sun, taken with another instrument of my own make. By several transits of the Sun and fixed stars, observed in the latter end of May and the beginning of June, I found I had brought it to go at the rate of mean solar time very nearly, *i. e.* losing but $1''$ in 14 days; and by transits of the Sun, it appeared to be $1''$,5 too slow for mean time, on the third of June. But by a mean
of

of 8 days observations of equal altitudes, taken in June and the beginning of July, the clock appeared $5''.2$ slower than was found by the transit-telescope, which was not perfectly adjusted; though, as its error continued always the same, it was very sufficient for determining the rate of the going of the clock. Hence, on the day of the transit, the clock was $6''.7$ or $7''$ too slow for mean time; and the observations that follow are corrected accordingly.

In watching for the first contact of Venus, I kept my eye on the Sun's edge where the contact was expected; keeping that point nearly in the center of my field; and the first impression which I saw (without any penumbra or atmosphere that I could perceive) was, at $7^h 12' 32''$ by my clock, or $7^h 12' 39''$ mean solar time. I looked particularly to see whether the rest of the Planet were visible while only part was on. If I saw that, it appeared as a portion of a larger circle: but I think I should not have observed it at all, if I had not looked for it. The dark part on the Sun did not appear to me with a smooth edge; and yet I could not discern any undulation in it: but the clouds obliged me to slide my smoked glasses so often, it interrupted me much. I then tried my strongest magnifying power of 110, but to no purpose; for there was an undulation on the Sun's edge, by that time, so great, that I thought it best to return to the former power of 55. Before the internal contact, at about $7^h 24'$, I lost the Sun entirely, and, though there were a few breaks in the clouds, he never appeared more that evening.

As to the eclipse the next morning, I saw that more perfectly; though at first there were many
flying



flying clouds; which, however, did not deprive me of any observation I was capable of making. It began here at $18^h 42' 17''$ by the clock, or $18^h 42' 24''$ mean time (a cloud had just passed; but I can be sure it was not visible $2''$ or $3''$ before); and it ended at $20^h 28' 1''$ by the clock, or $20^h 28' 8''$ mean time. I used the magnifying power 55, as I had done for the transit. As I have no micrometer, the best method I thought I could take to observe the phases of the eclipse, was by the immersions and emersions of the spots; which, if there were comparative observations in other places, might measure them better than any instruments I have. The scheme annexed shews the situation of the spots, as I judged of them by my eye; and the observations that follow refer to the numbers in the drawing, TAB. XVII. fig. 1.

Mean time.

h	'	''	
18	42	24	Beginning of the eclipse.
	44	37	Spot 1 immerses.
19	3	17	The nebula of spot 2 begins to immerge.
	4	47	Spot 2 quite immersed, but not the nebula.
	14	56	Spot 3 immerses.
	16	44	Spot 4 immerses.
	18	50	Spot 5 immerses,
	19	29	Spot 6 immerses.
	24	56	Spot 7 begins to immerge.
	25	22	Spot 7 immerses entirely.
	30	21::	Spot 1 immerses.

Mean time.

h / //			
19	33	50	Nebula of spot 9 begins to immerge.
	34	14	Spot 9 begins to immerge.
	34	51	Spot 9 immerges entirely.
	35	3	Spot 10 immerges.
	42	31	Spot 8 immerges.
	45	8	Nebula of spot 2 begins to emerge.
	45	52	Spot 2 emerges entirely.
	46	57	Nebula of spot 2 emerges entirely.
	59	22	Spot 4 emerges.
20	0	10	Spot 5 emerges.
	1	38	Spot 3 emerges.
	4	50	Spot 6 emerges.
	11	35	Spot 8 emerges.
	24	53	Spot 9 emerges.
	26	22	Spot 10 emerges.
	28	8	The eclipse ends.

The spots α and N° 8 were not visible (or overlooked) on the Saturday morning, when I took a drawing of the others; but I think I may be sure were both seen that evening. The spot α certainly was not visible the next morning, though N° 8 and all the rest were: and, as far as I can recollect, that was its true situation. But, as the Sun did not break out till a few minutes before 7, and was quite gone before the internal contact, I had no opportunity to watch it. The roundness and blackness of that spot, at first suggested a suspicion of its being a satellite of Venus: but as this is not corroborated by any other accounts, it is probably a mistake. The two wires at
right

right angles to each other, express a horary circle, and a parallel of the equator (the telescope moving in the plane of the equator); and serve to shew the situation of the spots in general. The other is an oblique wire, the corresponding one having been taken out a few days before, in order to leave the field more clear.

As to my latitude, I have tried it different ways with such instruments as I have; but what I trusted to most, were the zenith distances I took of α Cephei and α Cygni at one time; and η and ζ Ursæ Majoris at another (for some nights successively) with my transit telescope; from which I suspend a plummet, at a certain distance from the quadrant underneath, one of whose radii becomes thereby a tangent; which being graduated, I calculate by it the distance of stars near the zenith, and correct the errors of collimation by taking them both ways. I have likewise taken stars both ways, with my equal altitude quadrant, which indeed is a very imperfect instrument for that purpose: but, by calculating the mean, it turns out much the same as by the other method, viz. $52^{\circ} 40' 20''$ north.

The other figure (2) is a sketch of my transit-telescope, which you enquired after. The principle it goes upon is this: the center of the object-glass remains immovable, while the telescope (by cross-axes at right angles to each other, which intersect at that center) hangs as it were from thence, and has a free motion every way; and particularly its motion east and west is freed, as much as possible, from friction. At the focus, where the cross wires are,

it is suspended by a smooth even catgut, by which that end is let down to the altitude required. This catgut bears constantly against a deep angular groove that is cut round the end of the screw marked A; by which means that end hangs always from the same point at all altitudes; from whence, the centre of gravity of the telescope keeps it constantly in the same plane. This screw runs east and west; and therefore, when the telescope is drawn up to a horizontal position, serves to adjust it to the meridian, by directing the wires to a distant mark. The settling of that meridian mark is certainly the same in this as in other transit-instruments; and perhaps mine is not exactly right. But I suspect my instrument errs in another particular; that the line of collimation does not run parallel to the intersection of cross axes and the center of gravity; which it ought to do. This is owing to bad workmanship, which a skilful operator could easily set right; and, when adjusted, I should think would not be apt to vary, but would always continue to describe a true vertical, as indeed it has proved upon trial; having always, in this rude form, kept to the same errors at the same altitudes.

The rest of the drawing, I believe, explains itself: excepting the two curved pieces of paste-board between which the telescope is directed; which are designed to keep off the air that would make it vibrate; and do in effect take off also a great deal of false light in the day time, and answer the purpose of an illuminator at night.

The

The thought occurred to me while I was using Lord Charles Cavendish's meridian line; to which you see I thus adapt a telescope; and there being fewer particulars necessary to be attended to, when once rectified, I thought it would be more serviceable in a common building, than those transit-instruments which require so very steady a foundation.

East-Dereham,
June and July, 1769,

Francis Wollaston.

LXIX. *Observations of the Transit of Venus over the Sun, June 3, 1769; made by Mr. Owen Biddle and Mr. Joel Bayley, at Lewestown, in Pennsylvania. Communicated by Benjamin Franklin, L.L.D. F.R.S.*

Read Dec. 21,
1769.

ON the 26th of May, 1769, Joel Bayley and myself arrived at Lewestown (on Cape Hinlopen at the mouth of Delaware Bay), being ordered there, by the American Philosophical Society, held at Philadelphia, for promoting useful knowledge, to take an observation of the ensuing transit of Venus over the Sun's disc; and immediately set about fixing our time-piece, in a house (which we hired) on the south street of the town, where we were most likely to be free from interruption; and had an open view of the Sun and stars for our observations. We set a strong oak post in the ground, to which we screwed our clock case, resting the base of it on the ground, the face of it fronting a door which opens to the southward, so as to be convenient for us to hear the beat of the clock, where we intended to fix our telescopes and instrument for taking equal altitudes. We then set a post in the ground for the equal altitude instrument, which was
not

not so good as I wished, but much better than a Hadley's quadrant, as we found by experience, and the best we could procure in time for our purpose. It was a theodolite, with telescopic sights, in which there were cross hairs; it had a spirit level to adjust the plain of the instrument horizontally; and also one applied to the telescope parallel with its axis, and at right angles to the other spirit level. By means of these two levels and adjusting screws, we found we could adjust it very nearly, the instrument being a very good one of the kind. With this we set our clock, by taking equal altitudes of the Sun, which we corrected by the tables published in a pamphlet, intitled, *Instructions relative to the ensuing Transit of Venus, &c.* by the Rev. Mr. Nevil Maskelyne. The 27th, we got some good corresponding altitudes of the Sun, by which we set our clock; and took equal altitudes of some of the fixed stars, to prove the rate of our clock. After this it continued cloudy, with rain at times, and a high wind at north-east, till the 31st, when the clouds broke a little. During this time, we employed ourselves in measuring the distance of our place of observation from the stone fixed at the beginning, or east end, of the east and west line, which is the boundary between the three lower counties and Maryland, and is situate on Fenwick's Island; the latitude and longitude of this place being accurately determined by Messieurs Dixon and Mason.

The meridional difference of the latitude of the place of our observation, north from Fenwick's Island; at the beginning of the east and west line, as before described; being the easternmost end of the southern boundary.

boundary between the lower counties and Maryland, is $19^{\circ} 41'' 24'''$; and the meridional difference of longitude of the place of our observation, west from the point aforesaid, in Fenwick's Island, $4^{\circ} 45''$ of a degree. These data, with the latitude and longitude of Station Point, will determine exactly the place of observation.

June 2, the weather being clear, had good corresponding observations of the Sun.

June 3, the weather being remarkably fine, had good observations to set our clock. About 12 o'clock began to direct our glasses to the Sun, keeping it continually in the field from then to the time the observation was past. We agreed to watch our telescope one minute in turn, till about seven or eight minutes before the contact was expected, lest, by too steady an attention to the glasses, our sight should be impaired, so as to disable us from discerning the contact clearly. I had left my telescope the minute preceding the contact, intending to apply myself steadily to it, from the next minute, until the observation was past; and when the 48th second was called, I applied myself to the glass, and by the time three seconds were elapsed, I perceived, on that part of the Sun's limb where I expected the contact, a small impression, which proved to be the limb of Venus in contact with the Sun. All the limb of the Sun, which appeared at that time in the field of the telescope, had a small undulatory motion, which, I apprehend, was owing to dense vapours, which arose at that place, being near the sea. At Venus's first appearance to me, it was only like one of those waves on the limb or border of the Sun, increased in
so

so small a proportion, that I remained doubtful for several seconds, whether it was any thing else; thus it continued, making a deeper impression, with that tremulous motion, for about ten seconds, when the tremor where Venus was in contact ceased, and the indenture was truly circular, with an even termination.

My absence from the telescope, just before the contact occurred, deprived me of an opportunity of judging whether there was any appearance of an atmosphere preceding the western limb of Venus in contact; but when Venus had entered nearly one half of its diameter into the Sun's disc, my companion and myself saw a luminous crescent, which enlightened that part of Venus's circumference which was off the Sun, so that the whole of her circumference was visible, but did not continue so until the internal contact; and at the time of the first internal contact, the eastern or external limb of Venus seemed to be united to the Sun's limb by a black protuberance or ligament, which was not broke by the entrance of the thread of light, till four seconds after that the regular circumference of Venus seemed to coincide with the Sun's.

The telescope I made use of for viewing the transit, was a reflecting one, belonging to the Philadelphia Library Company, the speculums of which are $2\frac{1}{2}$ feet apart, and the lenses in the eye tube four inches apart; it was the least magnifying power that I used, as I found the tremulous motion too much magnified by the other power. The small one was in good order, and defined the Sun's limb, and spots on its disc, very clearly. I had applied a polar axis

to it, and made some rack-work, by which I could keep the same part of the Sun's limb in the field with ease; my companion was not so well provided with a telescope, the one he used being of Dollond's refracting glasses of $4\frac{1}{2}$ feet. This we fixed, with a ball and socket, to a post, by which it was easily directed to the Sun. Thus furnished, we found the contacts to take place as follows, reduced to mean time.

	h	m	s
Owen Biddle's External contact at	2	11	53
Internal one at	2	29	53
Joel Bayley's External contact was	2	12	15
lost by an accident, but seen by him,			
after it had taken place, at			
Internal ditto	2	29	53

It must be noted, the internal contact, given by Owen Biddle, is at four seconds before the thread of light had broken the dark ligament or protuberance, by which Venus's limb was united to the limb of the Sun, that being the time he estimated the two limbs to be in contact.

The internal contacts, we think, may be relied on; the external happening sooner than expected, occasioned a doubt at its appearance, which made the exact second of its appearance a little uncertain.

Signed,

Philadelphia,
June 9, 1769.

Owen Biddle.

Copy taken in haste, but the times examined by
Owen Biddle.

The

The times of the contacts of Venus with the limb of the Sun, as seen by Owen Biddle and Joel Bayley on Cape Hinlopen; with the true difference of latitude and departure of the place of their observation, from the Middle Point between Fenwick's Island and Chesapeake Bay, are as follows, viz.

	h	'	"	
External contact at	2	11	53	} mean time
Internal contact at	2	29	53	

The difference of latitude of the
place of observation, north of } 21,93 miles
Middle Point.

The meridian distance of the
place of observation, east of } 30,6356 miles
Middle Point.

The latitude and longitude of Middle Point were taken by Messieurs Dixon and Mason, and, as we suppose, communicated to the Royal Society, but we are not yet acquainted with it.

N. B. As we are not acquainted with the exact measure of a degree of latitude, agreeable to the above gentlemen's measurement, we have sent the difference of latitude and longitude in miles and decimal parts, as it may be reduced to greater certainty thereby.

Remarks by the ASTRONOMER ROYAL.

From the data given above, and the length of a degree of latitude, found by Messieurs Mason and Dixon, in these parts = 68,896 English miles, the difference of latitude of Lewestown and the Middle Point above mentioned (which is the same with the point A; see Messieurs Mason's and Dixon's measure of a degree, *Philos. Transact.* vol. LVIII. p. 276) is $19^{\circ} 53''$; but the latitude of the point A was found, by Messieurs Mason and Dixon $38^{\circ} 27' 34''$; therefore that of Lewestown is $38^{\circ} 47' 27''$ north; and the difference of its meridian, and that of the point A, or their difference of longitude, is $34' 0'' = 2' 16''$ of time, Lewestown being to the east. But if the difference of longitude of Lewestown east of the Stones on Fenwick's Isle be supposed truly given, in the former account, $5' 45''$ of a degree, then the difference of longitude of Lewestown and the point A will come out about $1'$ of a degree, or $4''$ of time less; for Mr. Dixon acquaints me, that the distance of the Stone on Fenwick's Isle, east of the point A, is 35 English miles wanting 100 yards. Now this is equal to $30' 26''$ of a great circle = $38' 51''$ of longitude; from which subtracting $5' 45''$, there remain $33' 6''$ for difference of longitude of Lewestown and Point A = $2' 12''\frac{1}{2}$ of time, or $3''\frac{1}{2}$ less than found before; and this latter I take to be nearest the truth. If this be so, Lewestown is very nearly under the same meridian with the southernmost part of the city of Philadelphia, or more accurately $13''$ of longitude, answering

answering to $1''$ of time, east of it. For, by Messieurs Maſon's and Dixon's measure of a degree, the point N (see *Philos. Transact.* Vol. LVIII. p. 276) is $2' 19''$ of longitude west of the point A; and N, by measurement, is 31 English miles due west of the southernmost part of the city of Philadelphia, answering to $35' 12''$ of longitude; from which subtracting $2' 19''$, there remain $32' 53''$, answering to $2' 11''\frac{1}{2}$ of time, for the difference of longitude of the southernmost part of Philadelphia, east of the point A. But Lewestown is found above to be $33' 6''$ of longitude $= 2' 12''\frac{1}{2}$ east of the point A, and consequently is $13''$ of longitude, or about $1''$ of time east of the southernmost part of the city of Philadelphia.

Nevil Maskelyne.

LX. *Observations of the Transit of Venus over the Sun, made at the Round Tower in Windsor Castle, June 3, 1769. By Daniel Harris, Master of the Royal Mathematical School in Christ's Hospital, and F. R. S. In a Letter to the Reverend Nevil Maskelyne, B. D. F. R. S. and Astronomer Royal.*

S I R,

Read Dec. 21, 1769. **I** HAVE taken the liberty to send you my observations on the transit of Venus, as likewise those made for ascertaining the going of the clock; all which I should have done last June, when I sent the times of the contacts, to the end that they might, if you thought them worthy, have been communicated to the Royal Society through your hands; but, waiting for an opportunity of using your transit-instrument, in order to ascertain the longitude of Windsor, which you was so obliging to lend me for that purpose, prevented; and am sorry still to say, that I have not once been able to make use of it, on account of the badness of the weather at the several times I took it down with me for that purpose. However, other methods have been

been made use of, and no pains spared for ascertaining that material point, in which I hope I have succeeded, as well as in making the other necessary observations; and shall be happy if they meet with your approbation, and in any way tend to assist in determining the grand point in question.

My situation for observing the transit, and making the previous observations, was extremely advantageous; all of which were made within hearing of the clock, which was a good regulator, fixed up in the Round Tower a fortnight before, by permission of the Governor, his Grace the Duke of Montagu; who was so obliging, upon my worthy friend Captain Alexander Schomberg's application to him, by letter, in both our names, immediately to give his consent, with strict orders to his servants to take care that we were not disturbed in making our observations, particularly on the day of the transit; which orders were most punctually obeyed; nobody being admitted into the Round Tower on that day but ourselves, and two others, as assistants to watch the clock; viz. the Reverend Doctor Bostock, Canon of Windsor, and the Reverend Mr. James Townley, Head-master of Merchant Taylors School; both gentlemen acquainted with the nature and use of astronomical observations.

The regulator was fixed truly perpendicular, and well fastened to the wall and floor of the room where the transit was to be observed, and observations made on its rate of going for several preceding days, both by Captain Schomberg, an officer, well acquainted with astronomical observations, and myself; by which we found that it lost at the rate of twenty

twenty seconds per day nearly. The greatest part of the altitudes taken for that purpose, with a good Hadley's quadrant, in a saucer of treacle and water, covered with your glass roof, when necessary, to screen it from the wind, which I found to be of great use, are herewith inclosed, with their corresponding times, &c. which have all been compared separately, in compliance with your request, both by Captain Schomberg and myself; and which I hope will be found to be satisfactory.

As to the longitude of Windsor Castle from Greenwich, which has so long engaged my attention, though I have not had an opportunity of ascertaining it with your transit-instrument, by the method of differences of azimuths, which I have long wanted to do, yet, notwithstanding, by a mean of several bearings of St. Paul's, taken from the corner of the terras, near the dial, with a good theodolite, and found to be $N. 82^{\circ} 30'$ from the true meridian, and the difference of latitude between that cathedral and Windsor Castle $2\frac{1}{2}$ geographical miles, think it may be very nearly determined; and in the following manner.

The latitude of St. Paul's, or, which is the same thing, of the Royal Mathematical School in Christ's Hospital, by the mean of a great number of observations, I make to be $51^{\circ} 30'\frac{1}{4}$ N. and by a mean of several double altitudes of the Sun, taken in a saucer of treacle and water, screened from the wind, I find the latitude of Windsor Castle to be $51^{\circ} 28'\frac{1}{4}$ N. the difference of latitude therefore between those two places is $2\frac{1}{2}$ geographical miles; with which, and the bearing of St. Paul's from the Castle $N. 82^{\circ} 30' E.$
(variation

variation $20\frac{1}{2}$ degrees allowed for), I make the difference of longitude between them (by Mercator) to be $30\frac{1}{2}$ miles, which is equal to $2' 2''$ of time: and recollecting your mentioning to me Dury and Bell's actual Survey of London and its environs for 30 miles, as of some use for the purpose, I have looked over it, and find by that, that the direct distance between Windsor Castle and St. Paul's, London, is 22 statute or measured miles: and by another Survey of the same kind, done by Kitchen, I find the distance between those two places to be very nearly the same. Therefore, with this distance of 22 miles, equal to 19 geographical ones; and the difference of latitude, by observation, between the two places $2\frac{1}{2}$ miles, I find the departure to be 18,8 miles, which gives 30,2 miles of longitude, equal to $2' 1''$ of time, agreeing within a second to the former method.

The difference of longitude, or difference of meridians, therefore, between the Round Tower Windsor Castle, and St. Paul's, London, I think I may venture to put at $30\frac{1}{2}$ miles, or $2' 2''$ of time; though I am persuaded, if any thing, it is rather more than less; to which if we add the difference of longitude in time between St. Paul's and Greenwich, which is $22''\frac{1}{4}$, it will give $2' 24''\frac{1}{2}$ of time for the difference of longitude between the Round Tower at Windsor Castle, and the Royal Observatory at Greenwich.

I cannot help observing, that the only inconvenient circumstance, during the time of observing the transit, was the wind; which, blowing rather hard, and directly into the telescope, together with the smallness

of the Sun's altitude at that time, made the limb so very ill defined and undulating, that it is possible there may be an error of five or six seconds, at least, in the time of the external contact : being anxious, therefore, of having the internal contact as exact as possible, I changed the magnifying power of my telescope from that of 125 times, recommended by yourself, to that of 55 times, the least of all, which succeeded beyond expectation ; for by this means that undulating motion of the Sun's limb was greatly reduced, though not entirely taken away, appearing much better defined than before, as did likewise that of the Planet Venus ; insomuch that the error, if any, in the time of the internal contact, by which I mean the completion of the thread of light formed by the Sun's circumference, cannot exceed three seconds. The observing of the two contacts with so different magnifying powers as that of 125 times and that of 55 times, must occasion some difference in the times, and duration between the two contacts, to what they would have been, had they both been observed with the same magnifying power ; which is to be allowed for.

Venus appeared remarkably protuberant on her upper limb, both before and at the time of her internal contact, which went gradually off soon after, but did not, though I earnestly attended to it, observe any thing like an atmosphere about her.

Be pleased, Sir, to accept of my best thanks for the use of your glass roof and transit-instrument, as likewise for your very obliging and useful communications, at different times, on the present subject,

subject, which I shall always have the most grateful sense of; and am, with real esteem,

SIR, Your much obliged,

and most obedient, humble servant,

Royal Mathematical School,
in Christ's Hospital,
Dec. 21, 1769.

Daniel Harris.

Times of the contacts of Venus with the Sun, as observed from the Round Tower, in Windsor Castle, by permission of his Grace the Duke of Montagu, June 3, 1769.

Latitude $51^{\circ} 28' \frac{1}{4}$ N. and longitude $2' 24'' \frac{1}{2}$ in time, W. from the Royal Observatory at Greenwich.

	By the clock.	Mean time.
	h ' "	h ' "
The external contact of Venus with the Sun,	7 4 30	7 06 14 p. m.
The internal contact at	7 22 38	7 24 22
Duration between the contacts, the clock being 1' 44" too slow for mean time,		0 18 08
* Venus's diameter measured 3 different times		0 59 $\frac{1}{2}$

Chords measured parallel to the equator.

Venus's western limb from the Sun's eastern limb, at $7^h 46' 04''$ mean time	3 42 $\frac{1}{2}$
Venus's eastern limb from the Sun's western limb, at $7^h 47' 04''$ mean time	15 16
Nearest distance of Venus's lower limb from the Sun's limb, at $8^h 0' 15''$, the last of her	2 49
The Sun's horizontal diameter (at $7^h 30'$)	31 42

Note, These observations were made with a good regulator, made by Binning, of Windsor; an 18 inch reflector, made by the late ingenious Mr. Short, and a double object glass micrometer, made by Dollond.

* The very same that I made it at the transit, 1761.

An account of the methods used to ascertain the going of a clock, fixed up in the Round Tower at Windfor Castle, in latitude $51^{\circ} 28\frac{1}{4}$ N. from the 30th of May to the 3d of June following, 1769.

♂ May 30 at $4\frac{3}{4}$ p. m.	{ By a mean of three double altitudes of the Sun's center, the limb not being well defined, taken with a good Hadley's quadrant, made by Adams, in a saucer of treacle and water, so placed within the room as not to be disturbed by the wind, after having worked each separately, I found the clock to be $3' 18''$ too slow for the Sun, and $0' 25''$ too slow for mean time.	
♀ May 31 at $8\frac{3}{4}$ a. m.	{ By a mean of four double altitudes, of the Sun's upper limb, after being worked separately, found the clock too slow for mean time	0 40
	{ And by a mean of 3 corresponding altitudes the same afternoon	0 48
♂ June 1 at $5\frac{1}{2}$ p. m.	{ * By a mean of 4 altitudes, worked separately, too slow	1 00
♀ June 2 at $8\frac{3}{4}$ a. m.	{ By 9 different altitudes of the Sun's upper limb, all worked separately, and taking the mean, found the clock too slow	1 16 $\frac{1}{4}$
	{ And, by a mean of 3 corresponding altitudes, the same day	1 17
♂ June 3 at $8\frac{3}{4}$ a. m.	{ By the mean of two altitudes only; the weather not permitting more, made the clock too slow for mean time	1 26
Ditto. at $4\frac{1}{4}$ p. m.	{ The same afternoon, the weather being extremely fine, by 4 more double altitudes of the Sun's upper limb, worked separately, and a mean taken, found the clock too slow for mean time	1 42

* Wound up the clock just before these altitudes were taken, which might affect it something, although a regulator.

By

$\frac{1}{2}$ June 3
 at $5^{\frac{1}{4}}$ p. m.

{	By 4 more double altitudes an hour after,	}	" "
	the weather being exceeding fine, tho'		" "
	windy, which obliged me to use the glass		" "
	roof, by working all of them singly,		" "
	and taking a mean, made the clock too		1 43
	flow for mean time		<u> </u>

By all which it appears, that the clock lost of mean time, from the 30th of May to the 3d of June, inclusive, at the rate of $19\frac{1}{2}$ seconds per day; and by the last set of observations, at the time of the transit, 1 second per hour.

Some of the foregoing double altitudes, with their corresponding times as shewn by the clock, and the results, are here subjoined.

¶ June 2 The weather very fine.

Times by clock.			Double alt.	Cl. too flow.	
h / "			°'s u. l.	' "	
At	8	25 55 a. m.	81 06 $\frac{1}{2}$	3	41
—	28	45	81 58 $\frac{1}{2}$	3	43
—	33	47	83 30 $\frac{1}{2}$	3	49
—	40	13	85 19 $\frac{1}{2}$	3	39
—	43	35	86 23 $\frac{1}{2}$	3	49
—	46	37	87 16	3	47
—	54	35	89 35	3	51
—	56	30	Ditto center	3	44
—	58	07	D° lower limb	3	51
				<u>9) - 414</u>	

Clock too flow for the Sun.

By the mean of all, clock too flow for the Sun
Equation of time

3 46
-2 29 $\frac{3}{4}$

Clock too flow for mean time

1 16 $\frac{1}{4}$

[43°]

Corresponding altitudes, taken the same day.

Times by cl. Dou. alt.

h ' "		o ' "		h ' "		' "		cl. too flow for m. time
At 8	5	18	74 50 up.limb	3	47	44	1 05½	
8	8	32	75 54½	3	44	01	1 20	
8	11	44	76 53	3	40	38	1 25½	
							<u>3)3 51</u>	

By a mean of the three, cl. too flow for m. time 1 17

June 3 The weather very cloudy, and likely to rain.

D. alt.

h ' "	o ' "	' "	cl. too flow for the Sun
At 8 15 47 a. m.	78 16 up. limb.	3 49	
8 18 17	79 02 ditto.	3 43	
		<hr/>	
Cl. too flow for the Sun, by the mean		3 46	
Equation of time		2 20	

Clock too flow for mean time. 1 26

Again,

At 4	11	38	p. m. 67 31½ up. limb.	4	02	clock too flow for the Sun.
—	14	21	66 44	3	51	
—	16	51	65 54	4	01	
—	19	01	65 14	4	03	
						<u>4)15 57</u>

By the mean, clock too flow for the Sun 3 59½

Equation of time

— 2 17

Clock too flow for mean time

1 42

Again,

[431]

Again,		Double alt.			
h	'	"	°	'	"
At 5	11	35	48	56	up. limb.
—	14	05	48	08 $\frac{1}{2}$	4 01
—	16	19	47	28	4 01
—	18	27	46	48	3 59
					3 57
					<hr/> 4)15 58
					<hr/> 3 59 $\frac{1}{2}$
					—2 16 $\frac{1}{2}$
					<hr/>
					1 43
					<hr/> <hr/>

Clock too slow for the Sun
Equation of time

Clock too slow for mean time

Received July 8, 1769.

LXI. *An Attempt to elucidate two Samnite Coins, never before fully explained. In a Letter to Mathew Maty, M. D. Sec. R. S. from the Rev. John Swinton, B. D. F. R. S. Custos Archivorum of the University of Oxford, Member of the Academy degli Apatisti at Florence, and of the Etruscan Academy of Cortona in Tuscany.*

I.

GOOD SIR,

Read Dec. 7, 1769. **T**HE first of the coins I propose to consider here is a Samnite denarius (see TAB. XVI. n. 1.) of Papius Mutilus, published by Sig. Olivieri and M. Pellerin, with the word SAFINIM on the reverse, in Samnite-Etruscan characters; an interpretation of which has, as I apprehend, been ineffectually attempted by the Marquis Scipio Maffei, Sig. Olivieri, Sig. Avvocato Passeri, and M. Pellerin. The other has the initial letter of the name of a town upon the reverse, indicating the place where it was struck. For that Etruscan
7 coins

coins are sometimes adorned with the initial letters of the names of towns, is a point that will admit of no dispute amongst the learned. No one will refuse his assent to this, who is apprized, that it has been allowed by Sig. (1) Olivieri and Sig. (2) Avvocato Passeri, the two greatest proficient in Samnite, or Samnite-Etruscan, literature that have hitherto appeared. What is now remarked has been also observed by Sig. Gori, in (3) the very valuable work here referred to.

These pieces, however, are of a species different from that of the coins of Papius Mutilus, Tiberius Venturius, and N. Luponius, of which I have largely treated in (4) some of my former papers. The first species appertains to cities, the other to the Samnite, or Italian, commanders, whose names they bear, for the most part, on the reverse. Hence it seems, at first sight, extremely probable, that the word *SAFINIM*, on the reverse of Papius Mutilus's medal, now before me, cannot be equivalent to *SABINI*, THE *SABINES*, or *SAMNITES*, THE *SAMNITES*, as some (5) of the most celebrated

(1) *Una lettera del Sig. Olivier. al Sig. Abate Barthelemy, &c.* p. 29. In Pesaro, 1757.

(2) Jo. Baptist. Passer. Pisarenf. *De Num. Etrusc. Pæstanor.* in *Symb. Litterar.* Vol. II. p. 30, 31. Florentiæ, 1748.

(3) Anton. Francisc. Gor. *Mus. Etrusc.* Vol. II. p. 428. Tab. cxcvi. cxcvii. Florentiæ, 1737.

(4) *Philosoph. Transact.* Vol. LII. Par. i. p. 28—39. Vol. LVIII. p. 253—261. & alib.

(5) Annib. degli Abati Olivier. *Dissertaz. sopr. alcun. Medagl. Samnit.* in *Sag. di Dissertaz. Accademich. &c. di Corton.* Tom. IV. p. 142, 146. in Roma, 1741. Scip. Maff. ap. Annib. degl. Abat. Olivier. ubi sup. Peller. *Second Supplem. aux Recueils des Medaill.* p. 1—14. A Paris, 1766.

Etruscan antiquaries of the present age have not scrupled to assert; but must be taken, agreeably to the nature and genius of such coins, for the name of one of the Italian generals, who distinguished himself in the Social war.

In support of the former opinion, it has been alleged, that the Etruscan plural masculine termination, at the time of the Social war, was IM, the very same (6) with the Hebrew; and that there is a most peculiar and necessary connection between the name SAFINIM and the (7) symbol on the reverse, which accompanies it. But neither of these reasons, which have been offered, or at least supposed to be true, by two very great men (8), will be admitted by the learned. For that the Etruscan plural masculine termination was not the same with the Hebrew, and consequently IM, in any of the cases, but at least similar to the Latin, if not the very same with it, in the later ages, of which that of the Social war was one, is clearly evinced by the Etruscan coins, with the words (9) ICVFINI, ICVVINI, or EVGVBINI, (10) TIANVR, TEANVR, or TEANOR, upon them, to omit other instances of the same kind, that might, with equal facility, be produced. And that the sym-

(6) Scip. Maff. ubi sup. M. Peller. ubi sup. p. 11, 12, 13.

(7) Peller. ubi sup. p. 5, 11, 12, 13, 14.

(8) Scip. Maff. & Peller. ubi sup.

(9) Anton. Francisc. Gor. *Mus. Etrusc.* Vol. II. p. 422. Florentiæ, 1737.

(10) *Numism. Antiq. &c.* Thom. Pembroch. et Mont. Gomer. Com. P. 2. T. 88. n. 3. *Philosoph. Transact.* Vol. LII. Par. i. p. 37.

bol on the reverse of the medal I am now upon has no necessary or immediate connection with the word *SAFINIM*, attending it, is, I conceive, plain from hence, that this very symbol appears on the reverses of two other Samnite coins, one of the Veturian, the other of the Luponian family, by (11) me heretofore explained, with the names of two Italian generals upon them. From whence, by parity of reason, and analogy, it will likewise farther follow, that the word on the piece now before me may be fairly presumed to be the name of a Samnite, or Italian, commander, famous for his bravery, and laudable conduct, in the Social war. This, I say, seems extremely probable, not only from the nature and genius of the coin itself, but likewise from the similarity and analogy it bears to other (12) coins, with the names of Italian captains most evidently upon them, attended by the same symbol that occurs on the medal which is the object of my attention here. But the truth of what is here advanced will, as I apprehend, even to demonstration, appear, if we consider, with proper attention, the legend on the reverse of a Roman denarius (see *TAB. XVIII. n. 2, 3.*) of the Servilian family, in conjunction with that on the reverse of a Samnite coin of Papius Mutilus, to whom appertains the piece I am now endeavouring to throw some light upon. The two first of these

(11) *Philosopb. Transact.* Vol. LVIII. p. 253—265. & Vol. LII. Par. I. p. 28—39.

(12) *Sag. di Dissertaz. Accademich. di Corton.* Tom. II. Dissertaz. 2. & Tom. IV. Dissertaz. 4. p. 133—149. *Philosopb. Transact.* Vol. LII. Par. i. p. 28—39. & Vol. LI. P. ii. p. 853—865.

medals are so perfectly (13) similar, that were not the characters with which they are adorned different, and the caps or helmets worn by two Castors visible only on one of them, they might absolutely be considered as duplicates of the same coin (see TAB. XVI. n. 2, 3.). Now the Roman denarius has preserved the legend SERVEILIM on the reverse, and the Samnite one I propose to elucidate here the inscription SAFINIM likewise on the reverse, in Samnite, or Samnite-Etruscan, letters. As therefore SERVEILIM is apparently equivalent to SERVEILI M., SERVEILIVS MARCI, or, in the Roman style, SERVEILIVS MARCI FILIVS; the legend SAFINIM may be considered as equivalent to SAFINI M., or SAFINIVS MARCI, which, in the Samnite mode of expression (14), answers to the Latin, or Roman, SAFINI M. F. that is SAFINIVS MARCI FILIVS. Wherefore it undoubtedly pointed at one of the Italian heroes, famous for his conduct and bravery in the war carried on, towards the decline of the seventh century of Rome, by the confederated Italian states, against the Romans.

That my interpretation of the legend on the reverse of the Roman denarius is agreeable to truth, we learn from the legend on the reverse of a similar denarius of the Servilian family (see TAB. XVIII, n. 4.), now in my small collection, which is formed of the letters P. SERVILIM F., that is P. SERVILIVS MARCI FILIVS. Whence, in conjunction with the Samnite denarius

(13) *Sag. di Differtaz. Accademich. di Corton.* Tom. IV. Differt. 4. p. 133, 134. In Roma, 1743.

(14) *Philosoph. Transact.* Vol. LII. P. i. p. 35.

(see TAB. XVII. n. 2, 3.), we may infer, that the word denoting SON, amongst the Samnites, was absorbed in the genitive case of the father's name, after the manner of the Greeks, as I (15) formerly supposed; and that, in this particular, they were even sometimes (16) imitated by the Romans (see TAB. XVIII. n. 3.)

With regard to the Safinian family, I shall beg leave to observe, that it was a family of pretty considerable note. We are told by (17) Sig. Olivieri, that C. Safinius had a hand in the seditions of L. Apuleius, which so much disturbed the repose of the republic, about the middle of the seventh century of Rome. The names of several members of this family occur in some of the Latin, or Roman, inscriptions, published by (18) Gruter and (19) Muratori, to omit what has been said on the same subject by other writers. Sig. Olivieri (20) supposes the Safinian family might not improbably have been of a Samnite origin, and the interpretation of the word SAFINIM given here seems to add no small weight to such a supposition. Nay, we learn from (21) some of the above mentioned inscriptions, that

(15) *Philosoph. Transact.* ubi sup. & not. (21).

(16) *Sag. di Dissertaz. Accademich. di Corton.* ubi sup.

(17) *Sag. di Dissertaz. Accad. di Corton.* Tom. IV. p. 144.

(18) Jan. Gruter. *Corp. Inscript. ex Recens. et cum Not. Joannis Georgii Grævii.* CCCLXXXVI. 4. CMLIII. 5. MLXXV. 2. & MXCII. 7.

(19) Lud. Ant. Murator. *Nov. Thesaur. Veter. Inscript. &c.* CCCIX. 3. MDXCIX. 6. MDCCXLIX. 2. & DCCCXLIX. 2. Vid. etiam *Ind. Univerf. Class. XVII.* Mediolani, 1742.

(20) *Sag. di Dissertaz. Accademich. di Corton.* Tom. IV. p. 146.

(21) Jan. Gruter. ubi sup. CMLIII. 5. Lud. Ant. Murator. MDCCXLIX. 2. & *Ind. Univerf. Class. XVII.*

M. or MARCVS, was a prenomén used by certain members of this family, which may be considered as an additional proof of the truth of what has been here advanced. It is true, we meet with no commander of this name in the history of the Social war. But then it is as true, that we have very little relative to this war handed down to us by the Roman historians. As the war of the Allies was exceeding bloody, and continued near four years; we may reasonably presume many of the Italian generals and principal officers to have been cut off in the course of it, and to have been succeeded by others, several of whose names have not been mentioned by those writers who have only just touched upon that war. Had the histories of the Social war written by Cornelius Sisenna, L. Lucceius, and others, whose works, excepting a very few small fragments, are now entirely lost, escaped the ravages of time, we might perhaps have met there at least with the name of this general, if not some account of his exploits in that war. But as the matter now stands, we must be solely obliged to the very valuable denarius I have been considering, which has preserved the name of a family not hitherto observed on antient coins, for the preservation of his memory, through such a series of ages as has elapsed between the commencement of the Social war and the present time.

I must not forget to remark, that the small (22) point between the two last letters of the word SAFINIM, on the coin, seems to divide the five preceding elements from the last letter M, and to an-

(22) *Sag. di Differtaz. Accademich. di Corton.* Tom. IV
P. 133, 139.

nounce M the initial letter of the prenomē of our (23) Safinii's, or Safinius's, father. I would rather account for the small point here mentioned in this manner than suppose it an accent, as Sig. (24) Olivieri does, since we find something similar to this on (25) Papius Mutilus's coins; such a point being clearly visible (26) there, between the element c, the initial letter of the prenomē of Mutilus's father and the word Paapi, that general's name. This observation will, perhaps, be allowed to bring no small accession of strength to the interpretation of the word SAFINIM here laid down, and, in its turn, to receive no inconsiderable support from that interpretation.

After maturely weighing the reasons above alledged, I would flatter myself, that the Royal Society will adopt the preceding interpretation, as much easier, more simple and natural, and even more consonant to the genius of such Samnite coins as that here explained, than any of those different (27) ones that have hitherto appeared, though proposed by four of the greatest (28) antiquaries this age, or indeed any other age, has produced. To that most illustrious body, therefore, I beg leave to submit, with the utmost de-

(23) *Philosoph. Transact.* Vol. LII. P. i. TAB. I. fig. 1. P. 34, 35.

(24) *Sag. di Dissertaz. Accademich. di Corton.* ubi sup. p. 139.

(25) *Philosoph. Transact.* Vol. LII. P. i. TAB. I. p. 28. *Sag. di Dissertaz. di Corton.* ubi sup.

(26) *Philosoph. Transact. & Sag. di Dissertaz. di Corton.* ubi sup. P. 133.

(27) Scip. Maff. ap. Annib. degl. Abat. Olivier. in *Sag. di Dissertaz. di Corton.* Tom. IV. p. 142, 146, 147. Jo. Bapt. Passer, ibid. Olivier. ibid. Peller, ubi sup. p. 11, 12, 13.

(28) *Iidem ibid.*

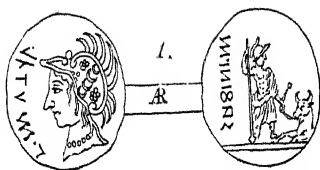
ference, what has been here advanced ; as well knowing, that their determination of the point in question will be received as decisive by the whole learned world.

II.

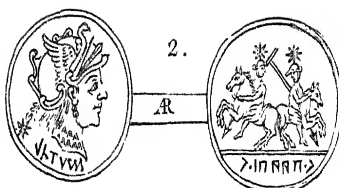
I find in M. Pellerin's third Supplement, published in the year 1767, a denarius (see TAB. XVIII. n. 6.), attributed by that very learned and ingenious gentleman to the city of Corfinium (29), the capital of the Peligni, where the deputies assembled, in order to regulate the operations of the war entered upon against the Romans, by the confederated Italian states, towards the decline of the seventh century of Rome. This notion, which, I believe, will not be contested by the learned, he founds upon the appearance of the letter c (30) on

(29) Peller. *Troisième Supplément aux six volumes de recueils des médailles de rois, de villes, &c. publiés en 1762, 1763, & 1764, à Paris, 1767.* p. 78—81. Pl. III. p. 90.

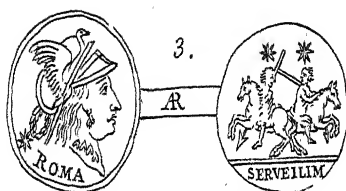
(30) Peller. *ubi sup.* That Etruscan coins are sometimes adorned with the initial letters of the names of towns, is a point that will be allowed by every one moderately versed in this branch of literature. I have one of these, with the Etruscan letter 7 (see TAB. XVIII. n. 7.), and the uncial mark, or single globule, upon it. On one side it exhibits an anchor, and on the reverse a wheel. I received this piece from an Italian gentleman, and have some reason to believe it might have been found in Tuscany. As therefore the anchor indicates it to have been struck in a maritime town, it may perhaps not improperly be attributed to Cosa, or Cossa, an antient and famous city of Etruria, seated near Telamon and Populonia, at a small distance from the sea, according to Strabo. This appears the more probable, as Etruscan coins of Telamon and Populonia have been actually discovered, and communicated to the learned world. Anton. Francisc. Gor. *Mus. Etrusc.* Vol. II. p. 428. TAB. CXCVI, CXCVII. Florentiæ, 1737. *Una Let-*
the



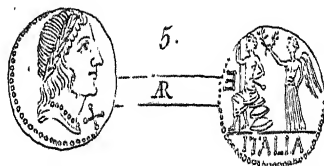
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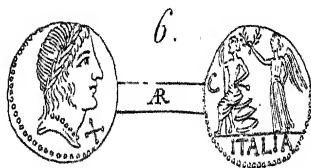
Sag. di Dißert. di Corton. Tom. IV. p. 133.



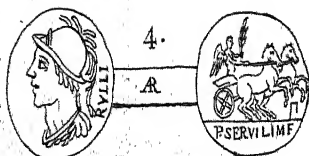
Sag. di Dißert. di Corton. Tom. IV. p. 133.



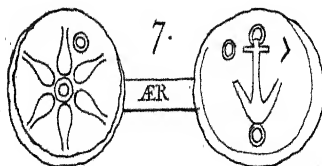
Penes Joannem Swinton, S.T.B. Oxonienf. R.S.S.



Peller. III. Supplem. p. 90.



Apud Joannem Swinton, S.T.B. Oxonienf. R.S.S.



Apud Joannem Swinton, S.T.B. Oxonienf. R.S.S.

the reverse; which he takes, with great reason, to be the initial letter of the word Corfinium, the name of that town. The coin of which I now send you a short account (see TAB. XVIII. n. 5.) agrees with that denarius in every particular, but the letter on the reverse; which is E, not C. But this is so far from overturning M. Pellerin's notion, that it will, at least in my opinion, strongly support, if not entirely confirm, it; which will, if I am not greatly mistaken, from what immediately follows, more clearly appear.

That these coins, and others similar to them, first appeared about the time of the Social war, must be allowed extremely probable, from the symbols on the (31) reverse which most of them exhibit, and has been observed by one of the greatest Etruscan (32) antiquaries of the present age. The letter E, on the reverse of my denarius, has a Samnite, Samnite-Etruscan, or oriental, direction, from the right hand to the left; which will, I conceive, notwithstanding the Roman letters in the exergue, sufficiently announce it a Samnite, or Samnite-Etruscan, coin. This also will, in some measure, be evinced by the character itself; which more resembles the ancient Etruscan form of E, than

tera del Sig. Olivier. al Sig. Abate Barthelemy, &c. p. 41, 42, 43. TAB. IV. n. 3, 4. In Pesaro, 1757. Sig. Canonic. Mazocchi. sopra l'Origin. de' Tirren. TAB. II. n. 15, 16. in Sag. di Dissertaz. Accademich. di Corton. Tom. III. p. 61, 62. In Roma, 1741. Strab. Geogr. L. V. p. 156.

(31) *Dissertaz. di Annibale degli Abati Olivieri sopra due medaglie Sannitiche in Sag. di dissertaz. Accademich. pub. let. nobil. Accadem. Etrusc. dell' antichiss. cit. di Cortona. Tom. II. p. 57. In Roma, 1738.*

(32) *Annib. degl. Abat. Olivieri, ubi sup.*

the later Roman, or Latin, figure of that element. Now the old name of the city, to which both the medals here mentioned may, as I apprehend, be assigned, was Corfinium, and the new one, given it by the confederated Italian states, Italica, as we learn from (33) Strabo. As the Samnites therefore and old Romans are known to have used E sometimes for I, the element E, on the reverse of my medal, may very well be supposed to have been the initial letter of the word Etalica, for Italica, the new name mentioned by Strabo (34). That the Samnites sometimes used E for I, we may infer from the word EMBRATVR, for IMPERATOR (35), on some of Papius Mutilus's Samnite coins, to omit others that might, with equal facility, be produced; and that the antient Romans likewise did, not infrequently, the same thing, from the authors (36) here referred to, is indisputably clear. From whence we may, as I apprehend, fairly collect, that M. Pellerin's denarius was struck about the time the league was formed, or concluded, in commemoration of it; and mine after the commencement of the war, which was the immediate consequence of that league. From what has been remarked, it is abundantly clear, that the pieces here mentioned not only receive a considerable share of light from Strabo, but likewise evidently support, in the point before us, the authority of that celebrated antient writer. Farther, as neither of these

(33) Strab. *Geograph.* lib. V. p. 241. Lutetiæ Parisior. 1620.

(34) Strab. ubi sup.

(35) Annib. degl. Abat. Olivieri, ubi sup. p. 61.

(36) Cic. *de Orat.* lib. III. c. 12. Varro *de Ling. Latin.* lib. V. Quintilian. lib. V. c. 6. Aul. Gell. *Noct. Attic.* lib. X. c. 24.

pieces was ever published, I believe, before the publication of M. Pellerin's third Supplement, they ought both to pass for a sort of inedited coins; and, as such, will have a place assigned them in the cabinets of the great, the curious, and the learned; which will, I hope, be deemed a sufficient apology for the trouble given, in the latter part of this paper, by,

GOOD SIR,

Your most obliged,

and most obedient,

humble servant,

Christ-Church, Oxon.
July 5, 1769.

John Swinton.

LXI. *Observation of the Transit of Venus, on June 3, 1769. In a Letter from John Leeds, Esquire, Surveyor General of the Province of Maryland, to John Bevis, M. D. F. R. S.*

Talbot County, in Maryland,
June 17, 1769.

S I R,

Read Dec. 21,
1769.

AS you will, I believe, have but few accounts from Maryland of the transit of Venus, I take the freedom to send you this.

Having no other instruments to observe with but a pocket watch and a reflecting telescope about twenty inches long, of Sterrup's make, on the third instant, when the Sun was on the meridian, I set my watch to 12, and at half an hour past one began to observe, keeping my eye to that part of the Sun's limb a little north of the vertex, where I expected Venus to come on; $2^h 10^{\frac{1}{2}}$ I perceived a small dent in the Sun's limb; $2^h 25^{\frac{3}{4}}$ Venus was totally within, so that the upper edge of the Sun and Venus seemed to touch. Being no better fitted for this business, I can give you no better account. I observed (and indeed from the calculation I expected it), that the

Planet

Planet entered deeper into the Sun's disc than that scheme in Lowthorp's Abridgment of the Philosophical Transactions, Vol. I. Plate IV. fig. 155, seems to shew.

My situation is, lat. $38^{\circ} 45'$, under a meridian, as near as I can guess, ten miles east of Annapolis, our chief town or city; and about twelve miles west of Cape Henry, at the mouth of Chesapeake Bay, as laid down by Messieurs Fry and Jefferson in their map of Virginia and Maryland. I do not know that what I have here mentioned can be of any use; but, if it be impertinent, you will be pleased to excuse it.

I am, SIR,

Your very humble servant,

J. Leeds.

LXII. *Experiments to prove that the Luminousness of the Sea arises from the Putrefaction of its Animal Substances. By John Canton, M. A. and F. R. S.*

Read Dec. 21, 1769. **I** SHALL not enter into the consideration of the several opinions of philosophers concerning the luminous appearance of the sea, as not one of them, that I know of, has been well supported; but I shall immediately relate a few experiments, which any person may very easily make, and which, I think, will be allowed to point out the true cause of that appearance, when compared with the descriptions given of it, by those who have accurately observed it.

EXPERIMENT I.

Into a gallon of sea-water, in a pan about 14 inches in diameter, I put a small fresh whiting, June 14, 1768, in the evening; and took notice that neither the whiting, nor the water when agitated, gave any light. A Fahrenheit's thermometer in the cellar, where the pan was placed, stood at 54 degrees. The 15th, at night, that part of the fish which was even with the surface of the water was luminous, but the water itself was dark. I drew the end of a stick
through

through the water, from one side of the pan to the other, and the water appeared luminous behind the stick all the way, but gave light only where it was disturbed. When all the water was stirred, the whole became luminous, and appeared like milk; giving a considerable degree of light to the sides of the pan that contained it; and continued to do so for some time after it was at rest. The water was most luminous when the fish had been in it about 28 hours, but would not give any light by being stirred, after it had been in it three days.

EXPERIMENT II.

I put a gallon of fresh water into one pan, and a gallon of sea-water into another, and also into each pan a fresh herring of about three ounces. The next night the whole surface of the sea-water was luminous without being stirred, but much more so when put in motion; and the upper part, of the herring, which lay considerably below the surface of the water was very bright. The fresh water was quite dark, as was also the fish that was in it. There were several very bright luminous spots on different parts of the surface of the sea-water; and the whole, when viewed by the light of a candle, seemed covered with a greasy scum. The third night, the light of the sea-water while at rest was very little, if at all, less than before; and when stirred, its light was so great, as to discover the time by a watch; and the fish in it appeared as a dark substance. After this, its light was evidently decreasing, but was not quite gone before the seventh night. The fresh water, and fish
in

in it, were perfectly dark during the whole time. The thermometer was generally above 60.

EXPERIMENT III.

Into a gallon of fresh water I put common or sea-salt, till I found by an hydrometer it was of the same specific gravity with the sea-water. In another gallon of fresh water I dissolved two pounds of salt: and into each of these waters I put a small fresh herring. The next evening the whole surface of the artificial sea-water was luminous without being stirred, but gave much more light when it was disturbed. It appeared exactly like the real sea-water in the preceding experiment, and its light lasted about the same time, and went off in the same manner*. The other water, which was almost as salt as it could be made, never gave any light. The herring, which was taken out of it the seventh night, and washed from its salt, was found firm and sweet; but the other herring was very soft and putrid; much more so than that which had been kept as long in the fresh water of the last experiment. If a herring, in warm weather, be put into ten gallons of artificial sea-water instead of one, the water will still become luminous, but its light will not be so strong.

N. B. The artificial sea-water may be made without the use of an hydrometer, by the proportion of

* Several river-fish, as the bleak, the dace, the carp, the tench, and the eel, were kept in artificial sea-water to putrefy, without producing any light that I could perceive: but a piece of a carp made the water very luminous, though the outside, or scaly part of it, did not shine at all.

four ounces avoirdupois of salt, to seven pints of water, wine measure.

From the second and third experiments it is evident, that the quantity of salt contained in sea-water hastens putrefaction; as the fish that had been kept in water of that degree of saltiness was found to be much more putrid than that which had been kept the same time in fresh water. This unexpected property of sea-salt was discovered by Sir John Pringle, in the year 1750, and published in the XLVIth volume of the Philosophical Transactions, with many very curious and useful experiments on substances resisting putrefaction; but the greatest quantity of salt there mentioned, is less than what is found in sea-water: it is probable, therefore, that if the sea were less salt, it would be more luminous. And here it may be worth remarking, that, though the greatest summer heat is well known to promote putrefaction, yet 20 degrees more than that of the human blood seem to hinder it: for, putting a very small piece of a luminous fish into a thin glass ball, I found that water of the heat of 118 degrees would destroy its luminousness in less than half a minute; which, on taking it out of the water, it would begin to recover in about ten seconds, but was never after so bright as before.

I shall now only add to these experiments the most circumstantial accounts I can find of the sea's luminous appearance. The Honourable Robert Boyle, in the third volume and 91st page, of Doctor Birch's edition of his works, says, "When I remem-

“ber how many questions I have asked navigators
 “about the luminousness of the sea; and how in
 “some places the sea is wont to shine in the night
 “as far as the eye can reach; at other times and
 “places, only when the waves dash against the ves-
 “sel, or the oars strike and cleave the water; how
 “some seas shine often, and others have not been ob-
 “served to shine; how in some places the sea has
 “been taken notice of, to shine when such and such
 “winds blow, whereas in other seas the observation
 “holds not; and in the same tract of sea, within a
 “narrow compass, one part of the water will be lu-
 “minous, whilst the other shines not at all: when,
 “I say, I remember how many of these odd phæ-
 “nomena, belonging to those great masses of liquor,
 “I have been told of by very credible eye-witnesses,
 “I am tempted to suspect, that some cosmical law
 “or custom of the terrestrial globe, or, at least, of
 “the planetary vortex, may have a considerable
 “agency in the production of these effects.”

Father Bourzes has given a still more particular
 account of the luminous appearance of the sea; part
 of which I have extracted from the third edition of
 Jones's Abridgment of the Philosophical Transactions,
 Vol. V. Part ii. p. 213. “When the ship ran apace,
 “we often observed a great light in the wake of the
 “ship, or the water that is broken and divided by the
 “ship in its passage. This light was not always
 “equal; some days it was very little, others not at
 “all; sometimes brighter, others fainter; some-
 “times it was very vivid, and at other times nothing
 “was to be seen. As to its brightness, I could
 “easily

easily read by it, though I was nine or ten feet
 above it from the surface of the water ; as I did
 particularly on the 12th of June, and the 10th of
 July, 1704. But I could read only the title of my
 book, which was in large letters. As to the ex-
 tent of this light, sometimes all the wake ap-
 peared luminous to thirty or forty feet distant
 from the ship ; but the light was very faint at any
 considerable distance. Some days one might easily
 distinguish in the wake such particles as were lu-
 minous from those that were not : at other times
 there was no difference. The wake seemed then
 like a river of milk, and was very pleasant to
 look on. At such times as we could distinguish
 the bright parts from the others, we observed
 that they were not all of the same figure. Some
 of them appeared like points of light ; others
 almost as large as stars, as they appeared to the
 naked eye. We saw some that looked like
 globules of a line or two in diameter ; and others
 like globes as big as one's head. It is not
 always that this light appears, though the sea
 be in great motion ; nor does it always hap-
 pen when the ship sails fastest : neither is it
 the simple beating of the waves against one an-
 other, that produces this brightness, as far as I
 could perceive. But I have observed, that the
 beating of the waves against the shore has some-
 times produced it in great plenty ; and on the
 coast of Brazil the shore was one night so very
 bright, that it appeared as if it had been all on
 fire.

“ The production of this light depends very
 “ much on the quality of the water ; and, if I
 “ am not deceived, generally speaking, I may as-
 “ sert, other circumstances being equal, that the
 “ light is largest when the water is fattest, and fullest
 “ of foam ; for, in the main sea the water is not
 “ every where equally pure ; and sometimes if one
 “ dips linen into the sea, it is clammy when it is
 “ drawn up again. And I have often observed,
 “ that when the wake of the ship was brightest,
 “ the water was more fat and glutinous ; and linen
 “ moistened with it, produced a great deal of light,
 “ if it were stirred or moved briskly. Besides, in
 “ sailing over some places of the sea, we find a
 “ matter or substance of different colours, some-
 “ times red, sometimes yellow. In looking at it,
 “ one would think it was saw-dust : our sailors say
 “ it is the spawn, or seed of whales. What it is,
 “ is not certain ; but when we draw up water, in
 “ passing over these places, it is always viscous and
 “ glutinous. Our mariners also say, that there are
 “ a great many heaps or banks of this spawn in the
 “ north ; and that sometimes in the night they ap-
 “ pear all over of a bright light, without being
 “ put in motion by any vessel or fish passing by
 “ them.

“ But, to confirm farther what I say, videlicet,
 “ that the water, the more glutinous it is, the
 “ more it is disposed to become luminous ; I shall
 “ add one particular which I saw myself. One day
 “ we took in our ship a fish, which some thought
 “ was a boneta. The inside of the mouth of the

“ fish

“ fish appeared in the night like a burning coal;
 “ so that, without any other light, I could read by
 “ it the same characters that I read by the light
 “ in the wake of the ship. Its mouth being full
 “ of a viscous humour, we rubbed a piece of wood
 “ with it, which immediately became all over lu-
 “ minous; but, as soon as the moisture was dried
 “ up, the light was extinguished.

“ I leave it to be examined whether all these
 “ particulars can be explained by the system of such
 “ as assert, that the principle of this light consists
 “ in the motion of a subtle matter, or globules,
 “ caused by a violent agitation of different kinds
 “ of salts.”

LXIV. *A Series of Astronomical Observations made at the Observatory of the Marine at Paris, to wit, 1°. Observations of Jupiter's Satellites in the Years 1767 and 1768. 2°. Observations on the Shadows of Jupiter's Satellites. 3°. On the Variation of the Belts on the Disc of that Planet. 4°. Observation of a Spot on the Disc of the 3d Satellite. 5°. Observation of the Belts of Saturn. 6°. Observation of the Moon's Passage over the Pleiades, in 1767. 7°. Observation of a partial Eclipse of the Moon, January 3, and of a total one, December 23, 1768. 8°. Observations of Two Auroræ Boreales, August 6, and December 5, of the same Year. By M. Mefsier, Astronomer of the Marine, F. R. S. and of the Academies of Holland and Italy.*

Observations of Eclipses of Jupiter's 4 Satellites, made at Paris, in the Observatory of the Marine, in the Year 1767, with an excellent Gregorian Telescope of 30 Inches Focus, the great Speculum 6 Inches Diameter, and the magnifying Power 104 Times.

1767		True time		
		h	"	
Jan.	25	11	18 17	I. 4. Sky serene. γ well defined, the satellite 12' losing its light, during the two last of which it was extremely small. γ 28° high in the east. Good observation.

1767	True time			
	h	'	"	
Jan. 25	15	9	57	E 4. Sky serene. μ 47° high in the west. Good observation. In both I kept μ without the field of the telescope, to view the satellite the better.
26	15	34	34	I. 1. Clear sky. μ well defined, 46° high in the west. Good observation.
29	10	18	7	I. 2. Serene. μ well defined, 22° high in the east. Good observation.
Febr. 2	17	27	13	I. 1. μ well defined, 31° high in the west. The satellite extremely small for a minute. Good observation.
19	17	57	56	I. 2. Sky serene round μ , being 16° high westward. Good observation.
March 8	8	31	35	I. 1. Clear sky. The satellite disappeared touching the disc of μ . Doubtful to some seconds, the opposition being this night at $10^h 49' 4''$. At $10^h 29' 4''$ the satellite begins to appear at μ 's eastern limb; at $10^h 51' 34''$ half out; at $10^h 51' 34''$ quite separated from μ 's limb.
20	7	7	26	E. 2. Clear sky about μ . The satellite emerged at $\frac{2}{3}$ of the Planet's diameter. μ 25° high eastward. Good observation.
April 9	7	29	50	E. 1. Serene; but the observation doubtful to some seconds, from the proximity of μ to the Moon and the horizon.
10	14	58	18	E. 2. Clear sky; but μ near the horizon and the Moon. Observation doubtful to 5 or 6 seconds. The satellite emerged at $\frac{2}{3}$ of a diameter from the Planet. μ 15° high westward.
28	9	32	10	E. 2. Clear sky. μ well defined. The satellite emerged $\frac{2}{4}$ of a diam. from μ , which was 47° high. Good obs.

1767	True time			
	h	'	"	
April 29	10	13	29	E. 3. Clear sky. γ well defined. The satellite already emerged, perhaps for $\frac{3}{4}$ of a minute; it recovered not its full light till 4' after. The emergence was a diameter from γ .
May 5	12	9	39	E. 2. Clear sky. γ well defined. Satellite emerged at more than a diameter from γ , being then 25° high westward. Good observation.
6	11	17	37	I. 3. Sky much clouded. γ seen through thin clouds; but I think the observation may be depended on to 10 or 12 seconds.
Dec. 21	18	34	32	I. 3. Clear sky. γ well defined. Satellite entered the shadow 1 diameter from γ , after having been 2' extremely small. Good observation.
23	14	43	47	I. 1. Clear sky; but γ near the horizon, and ill defined. However, I esteem the observation a good one. The satellite emerged $\frac{2}{3}$ of a diameter from γ , being then 12° above the horizon.
March 15	12	41	48	E. 1. At Colombes, near Paris, $20\frac{2}{3}$ seconds of time west of the Royal observatory. The satellite out of the shadow, with a ten feet refractor of Dollond, magnifying 120 times. Good observation.
April 16	9	27	3	E. 1. At Colombes; serene sky; the satellite emerged at $\frac{1}{2}$ diameter from γ ; with a Dollond's refractor of ten feet.
May 30	9	14	47	E. 2. At Calais; sky serene round γ , with a $3\frac{1}{2}$ feet refractor of Dollond, with a triple object-glass. The satellite emerged 1 diameter from the Planet, a little above the 4th satellite. Good observation.

1767	True time			
	h	'	"	
June 1	9	51	40	E. 1. At Calais; sky serene; γ well defined. The satellite emerged at the distance of $\frac{1}{2}$ a diameter. Same refractor. Good observation.
8	11	24	4	I. 4. At Dunkirk; serene sky; but γ near the horizon; the belts, however, appeared plain, and I think it a good observation. The satellite was more than 6' losing its light. Dollond's $3\frac{1}{2}$ feet refractor.
	11	47	54	E. 1. At Dunkirk; serene sky; but γ very near the horizon, and ill defined. Doubtful observation.
1767				Observations on the shadow of Jupiter's satellites, and the variations of his belts, at the Observatory of the Marine, with a Gregorian reflector, of 30 inches focus, magnifying 104 times.
Febr. 19	12	58	46	The shadow of the first satellite appeared entirely on γ 's disc, and proceeded along the upper part of the middle belt.
	13	18	3	The satellite itself entered half way on γ 's disc, following its shadow on the same belt.
	14	1	7	The shadow of the fourth entirely entered on γ 's disc, and running along the lower edge of γ 's upper belt.
	15	3	57	The shadow of the first satellite in internal contact with γ 's limb.
	15	7	27	The shadow of the first quite off γ 's limb.
	15	25	23	The first beginning to go off the disc.
	15	29	53	The first gone off, but still in external contact with the limb.
	15	29	53	The shadow of the fourth seems advanced one half of its path over the disc.
	16	29	43	The shadow of the fourth seems to be two of its own diameters from γ 's limb.

1767	True time	
	h ' "	
Febr. 19	16 42 41	The shadow of the fourth, at least, one of its own diameters from \mathcal{U} 's limb.
	16 54 39	The shadow of the fourth looks oval, and touches \mathcal{U} 's limb.
	17 5 35	The shadow of the fourth quite off \mathcal{U} 's western limb.
	17 5 35	The fourth satellite almost touches \mathcal{U} 's eastern limb.
	17 13 34	The fourth satellite entering on \mathcal{U} 's disc, and forming an indentation on his limb.
	17 35 32	The fourth satellite quite entered, but not so visible on the disc as the first.

The shadow of the fourth seemed larger than that of the first; it was also more perceptible; owing, perhaps, to the shadow of the first running along the upper side of the middle belt, which was darker and more conspicuous than the upper belt, where the shadow of the fourth was. It was an easy matter to form a judgment of their differences, as the two shadows were at the same time seen on \mathcal{U} 's disc. At 6 $\frac{1}{4}$ ^h in the morning the sky became clouded, so that the egress of the fourth from the disc could not be observed.

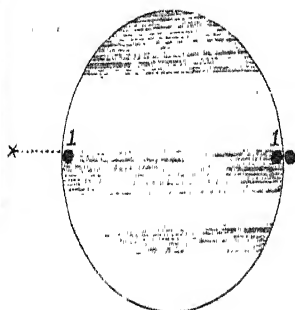
The figure n^o 1 (TAB. XIX) represents \mathcal{U} 's disc at the moment of the entry of the shadow of the first satellite; the upper part was shaded, but less sensibly than the middle belt, which was blackish, and of a darker hue in some parts than in others. Below this middle belt, another was visible, which terminated at about two-thirds of \mathcal{U} 's diameter. It was narrow, but as distinguishable as the middle belt.

The fig. n^o 2 represents the shadow of the fourth satellite, which passed along the upper belt less distinct than the middle one. At 3^h 11" the lower narrow belt was not to be seen; the middle belt was also altered, being of various shades, and different from that represented in fig. 1. at the time of the immersion of the shadow of the first satellite.

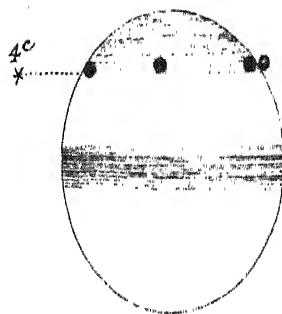
The fig. n^o 3 represents the shadows of the first and fourth satellites, viewed at the same time.

The fig. n^o 4 represents \mathcal{U} 's belts: the upper belt was the same as in fig. n^o 1; the middle belt was likewise the same; the

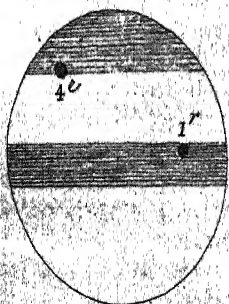
I.



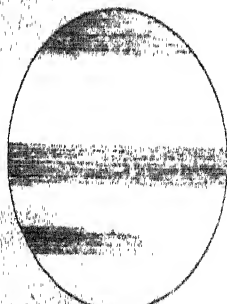
II.



III.



IV.



the narrow belt, which had disappeared, appeared again on the eastern side. At $4^h 30'$ it was advanced $\frac{1}{4}$ of the diameter. At $5^h 58'$ it extended almost from one limb to the other, as narrow and sensible as in fig. n^o 1.

The 6th of September, 1760, having computed the ingress of the third satellite on \mathcal{U} 's disc, I viewed the Planet with a Gregorian reflector of 30 inches focus, magnifying 104 times. At $7^h 4''$ I perceived at the center of the Planet, on the lower belt, a black spot, pretty round, and nearly of the size of the shadow of the first satellite. I guessed this spot to be the shadow of the third. I observed its progress, and being got on $\frac{2}{3}$ of \mathcal{U} 's disc, at $8^h 13'$ I perceived the shadow of the third just entered on \mathcal{U} 's eastern limb, and was larger than that I observed before, which made me to imagine, that the first shadow might be a spot on the very disc of the third satellite. I went on with my observations, and found, that the nearer this shadow approached the western limb of \mathcal{U} , the more it was diminished in size; and I lost sight of it before it had got to the Planet's limb. Lastly, at $9^h 26'$, the third satellite was half emerged, and formed an indentation on the disc. I was then well satisfied, that the observed spot was on the disc of the third satellite; and I took notice, that this satellite, when quite emerged, was not so luminous as usual.

The 28th of March, 1766, having viewed Saturn with the same achromatic reflector of 10 feet 7 inches focus, I perceived on his globe two darkish belts; they were indeed extremely faint, and difficult to be discerned, directed, however, in a right line parallel to the longest diameter of Saturn's ring.

The several observations here recited are extremely nice; and it were to be wished that astronomers, concerned in observations, might be accommodated with achromatic telescopes, of the most perfect construction; as such are the only instruments whereby a great knowledge of the celestial bodies can be acquired, for the improvement and perfection of astronomy.

Observations of the Moon's Transit over the Pleiades, in 1767,
at the Observatory of the Marine.

1767	True time			
	h	'	"	
Sept. 12	14	11	42	Imm. of * <i>b</i> , Electra, at the illuminated limb of the Moon.
	15	0	49 ¹ / ₂	Imm. of * <i>d</i> , Merope.
	15	6	8	Imm. of * <i>e</i> , Maia, near the limb, as it were shaving it.
	15	14	56	Emerf. of * <i>s</i> , Celeno, doubtful to 5 or 6 seconds, from thin clouds.
	15	24	49	Emerf. of * <i>b</i> , Electra. Good.
	15	31	11	Emerf. of * <i>n</i> , Alcyone. Good.
	15	47	39	Emerf. of * <i>c</i> , Maia. Good to a second.
	15	54	31	Emerf. of * <i>d</i> , Merope. Doubtful.
				Looking into the telescope, was visible; though but a small time emerged.
	16	51	11	The Moon clear of clouds, <i>n</i> appeared; it had emerged a few minutes.
Nov. 6	17	2	10	Conjunction of * <i>f</i> Atlas. It passed very near the Moon's limb, being only 10 parts of the micrometer from it, equal to 9 seconds.
	11	9	50	Imm. of * <i>d</i> , Merope. Doubtful to 2 or three seconds, from the great light of the Moon.
	11	57	57 ¹ / ₂	Imm. of * <i>n</i> , Alcyone. Good to a second.
	12	11	55 ¹ / ₂	Emerf. of * <i>d</i> , Merope. Good to a second.
				The * <i>s</i> was effaced by the light of the Moon.
	12	31	59	Imm. of * <i>f</i> , Atlas. Good.
	12	45	50	Emerf. of * <i>n</i> , Alcyone. It was already out, perhaps half a minute.
	13	42	25 ¹ / ₂	Emerf. of * <i>b</i> , Pleione. The immersion could not be observed, for the great light of the Moon. The emersion good to a second.

1767	True time			
	h	'	"	
Nov. 6	13	43	28½	Emerf. of * <i>f</i> , Atlas. Good to half a second.
Dec. 31	4	32	57	Imm. of * <i>d</i> , Merope.
	4	40	18½	Emerf. of * <i>d</i> , Merope.
	5	14	0	Conjunction of * <i>n</i> , Alcyone 280 parts of the micrometer = 4' 5" from the Moon's upper limb.
	5	27	58	Imm. of * <i>f</i> , Atlas, into the obscure limb of the Moon. Doubtful to 2".
	6	20	20	Emerf. of * <i>f</i> , Atlas, from the illumined limb of the Moon against Cleomedes. Good. * <i>b</i> could not be observed for moon-light.

Observations of the eclipses of Jupiter's satellites, made at the Observatory of the Marine at Paris, in the year 1768, with a Gregorian reflector, of 30 inches focus, magnifying 104 times.

1768	True time			
	h	'	"	
Jan. 31	12	53	22	Imm. 1. Sky serene. μ well defined. Good observation.
Feb. 2	18	4	36	Imm. 3. Sky serene. The satellite extremely small for two minutes.
	14	16	39 24	Clouds covered μ . The first satellite had then lost much of its light: 28" after μ re-appeared, but the first satellite was no longer seen. I put the immersion at 16 ^h 39' 50".
March 1	14	57	56½	Imm. 1. Sky serene. The Moon above the horizon, which did no great harm. μ was well defined; the satellite very small for 45", it entered the shadow at ½ of a diameter from μ . μ 35° high westward. Good observation.
10	11	22	15	Imm. 1. Sky serene. μ well defined. For 30" the satellite was very small. It entered the shadow at ½ diam. from μ . μ 26° high east. Good observ.

1768 .	True time	
	h	m
April 3	0 55 34	Imm. 2. Sky serene round γ , which was well defined. The satellite entered the shadow near γ 's limb. I esteem it a good observation.
27	8 35 11 $\frac{1}{2}$	Emerf. 1. Sky serene. γ pretty well defined: the Moon, though near him, did not much incommode. The satellite emerged $\frac{1}{2}$ diameter from γ , then 32° high eastward. Good observation.
May 4	10 31 0	Emerf. 1. Sky serene. γ well defined. Satellite emerged $\frac{1}{2}$ diameter from γ , then 36° high, having lately passed the meridian. Good observation.
5	13 7 51	Emerf. 2. γ , among interstices of clouds, was not well defined; the belts not distinct. Satellite emerged $\frac{1}{2}$ diameter from γ , then 22° high. Pretty good observation.
11	12 26 15	Emerf. 1. γ well defined. Good observation.
20	8 49 54	Emerf. 1. Sky pretty clear, and γ well defined. With a Dollond's 5 feet refractor with a double object glass. Satellite emerged $\frac{1}{2}$ diameter from γ . The satellite very small, and the observation good.
27	10 03 50	Imm. 3. Sky serene. γ well defined. Satellite continued very small for $3'$. It entered the shadow at $\frac{1}{2}$ diameter from γ , then 32° high westward. Good observation.
	10 43 14	Emerf. 1. Sky still serene, and γ well defined. Satellite emerged $\frac{1}{2}$ diameter from γ , then 30° high westward. Good observation.
	12 5 42	γ just free of clouds. The third satellite appears in nearly its full lustre.
June 3	12 37 39	Emerf. 1. Sky serene; but γ too near the horizon; and, being among vapours, ill defined. Satellite emerged

1768	True time	
	h ' "	
June 19	10 53 43	$\frac{1}{2}$ diameter from μ , then 10° high. Good to a few seconds. Emerf. 1. Sky partly clouded, but μ pretty bright at the time of observation. Satellite emerged $\frac{1}{2}$ diameter from μ , then 17° high. Good to a few seconds.
July 5	9 8 5	Emerf. 1. Sky clear. μ well defined. The emersion at $\frac{1}{2}$ diameter from μ , 22° high westward. A good observation.
28	9 20 20	Emerf. 1. Clear sky, but μ only 5° high westward. The Planet ill defined, though the belts were plain enough. A doubtful observation.

Observation of a partial Eclipse of the Moon, on the night between the 3d of January, 1768, at the Observatory of the Marine at Paris, with a four Feet Newtonian Reflector, magnifying 66 Times, armed with a Micrometer.

1768	True time	
	h ' "	
Jan. 3	11 50 20	The Moon's centre passed the meridian.
	12 8 38	S. II. passed. $1^\circ 27' 41''$, the difference between the altitude of the Moon's upper limb and that of the star, the star being lower.

1768.	Un eclipsed part of the Moon.	The part eclipsed.	
January 3.			
True time.			
h ' "	h ' "	h ' "	
15 14 40			The penumbra sensible.
15 18 49			Very strong.
15 22 48			The eclipse begins.
15 28 47			Immersion of Tycho.
15 30 22	27 15	4 5	
15 36 46	25 52	5 28	
15 49 44	22 43	8 37	
15 54 43	22 8	9 12	

1768. January 3. True time.	Uneclipsed part of the Moon.	The part eclipsed.	
' "	' "	' "	
16 0 12	20 46	10 34	
16 4 40	20 14	11 6	
16 9 34			Fracastorius enters the shadow.
16 10 40			The same entirely in the shadow.
16 10 40			Mare Nectaris enters the shadow.
16 12 39	19 24	11 56	
16 15 39	31 20		The Moon's diameter.
16 18 38	18 47	12 33	
16 21 38			Mare Nectaris half entered.
16 23 8	18 40	12 40	
16 24 38			Mare Nectaris totally in the shadow.
16 26 37	18 31	12 49	
16 26 37			Langrenus is entered into the shadow.
16 31 36	18 18	13 2	
16 35 35			Mare Imbrium nearly half entered.
16 38 34	18 20	13 0	
16 40 34	18 22	12 58	
16 46 34	18 38	12 42	
16 48 33	18 49	12 31	The Moon in a mist, the shadow ill defined.
17 3 30	20 15	11 5	The Moon pretty clear, Mare Humorum got out of the shadow 7' or 8'.
17 5 0			Mare Nubium clear of the shadow.
17 7 20	21 17	10 3	
17 12 28			Mare Nectaris begins to emerge.
17 14 28	22 12	9 8	
17 14 58			Tycho half out.
17 16 57			Mare Nectaris half out.
17 16 57	23 9	8 11	
17 17 27			Tycho clear of the shadow.
17 21 27	24 13	7 7	
17 25 26			Fracastorius out of the shadow.
17 26 26			Mare Nectaris out of the shadow.
17 27 56	25 18	6 2	
17 34 55	26 57	4 23	

1768. January 3. True time.	Parts en- lightened.	Parts e- clipsed.	
h ' "	' "	' "	
17 36 25			Mare Imbrium out of the shadow, which did not cover one half of it.
17 39 53	28 57	2 23	
17 43 23	29 42	1 38	
17 43 53			The Moon's limb begins to be visible.
17 45 53	29 59	1 21	
17 46 54			End of the eclipse.
17 51 22	31 32		Moon's diameter.
17 55 20			Much penumbra still left.
17 57 30			Now as before the eclipse began.
18 14 20			Now little or nothing sensible.

During the night of the eclipse the sky was greyish, and overspread with thin clouds; but the stars might be discerned. The shadow pretty well defined, and would have been much more if the sky had been clear. The cold was considerable all night long, the thermometer marking 11 degrees below 0, on Reaumur's scale.

Observation of the total Eclipse of the Moon, the 23d of December, 1768, in the Evening, at the same Observatory of the Marine, with the same Newtonian Reflector of $4\frac{1}{2}$ feet. The Sky serene during the 23d Day, but some Clouds in the West during the Eclipse; in the East serene, with a small Mist, which was favourable; the Shadow well defined. I had no View of the Moon, at the Horizon, because of elevated Land Objects.

1768. Dec. 23. True time.	Parts en- lightened.	Parts e- clipsed.	
h ' "	' "	' "	
4 27 0			The Moon beginning to appear over the church of Nôtre Dame, seeming to the bare eye half way eclipsed. Tycho already some minutes emerged.

1768, December 23. True time, h ' "	Parts en- lightened. ' "	Parts e- clipsed. ' "	
4 35 36	15 4	17 29	
4 39 3	17 6	15 27	
4 41 0	18 16	14 17	
4 43 7	19 58	12 35	
4 44 30			Mare Serenitatis quits the shadow.
4 44 39			Dionysius quits the shadow.
4 46 14	21 41	10 52	
4 47 54			Mare Serenitatis half out.
4 49 34	23 20	9 13	
4 49 48			Mare Tranquillitatis half out.
4 51 8			Mare Serenitatis quite out.
4 52 18	24 31	8 2	
4 54 18			Mare Tranquillitatis clear of the shadow.
4 55 26	26 40	5 53	
4 56 29			Mare Imbrium half out.
4 58 4			Mare Crisium half out.
4 59 7	28 57	3 36	
5 0 34			Mare Crisium and Mare Imbrium quite out.
5 2 10	30 52	1 41	
5 2 50			The Moon's limb beginning to be discernible.
5 3 36			The end of the shadow, or of the eclipse.
5 4 6			Yet more certain.
5 5 6			Strong penumbra.
5 5 46			Yet very sensible.
5 6 45	32 32		The Moon's diameter.
5 8 45			The penumbra still remains.
5 9 45	32 34		The Moon's diameter.
5 15 44			A small matter of the penumbra still remaining.

Read November 16, 1769.

LXV. *Astronomical Observations made by Order of the Royal Society, at Prince of Wales's Fort, on the North-West Coast of Hudson's Bay. By William Wales and Joseph Dymond.*

Mem. The Thermometer marked A was hung within the southern, or lower Observatory; in such a Part as we judged would be least affected by the Fire; close to, and with its Ball exactly of the same Height with, the Quicksilver in the Basin of the Barometer: That marked B was hung without Doors, on the north Side of the Observatory.
The Floor of the Observatory might be above 50 Feet above the Level of the Sea at Low-water Mark.

1768	Equal altitudes. Times by the clock.				Zenith distance	Baro- meter.	Thermo- meters.		Phænomena and Circum- stances.	
	Lower Wire	Middle Wire	Upper Wire	Passed the Meridian.		Inches	A	B	Ob- server	
Septemb.	" " h ' "	" " " " "	" " " " "	" " " " "						
♂ 14	17 15	19 20	51 24 33	23 49 18	75 40	26,56	38½	38	W.	{ ☉'s U. L. } easterly ☉'s L. L.
♂ 15	15 59	4 12	18 8 37		75 40	29,61	46	42½	W.	{ ☉'s L. L. } westerly ☉'s U. L.
☉ 18	At noon wound up the clock.								D.	
♂ 19	14 54	19 18	36 22 10	23 54 7	78 13	29,97	37	34	D.	{ ☉'s U. L. } ☉'s L. L.
	19 15	25 56	26 37		76 0	29,98	37	34	D.	{ ☉'s U. L. } easterly ☉'s L. L.
	33 8	36 55	40 39							
	37 37	41 25	45 11							
♂ 20	9 36	4 12	18 8 37		76 0	30,07	49	44	D.	{ ☉'s U. L. } ☉'s L. L.
	14 3	4 10	10 6 32	23 55 00	78 13	30,07	49	44	D.	{ ☉'s U. L. } westerly ☉'s L. L.
	28 1	24 16	20 37							
	32 19		25 1							
	36 56	19 40	44		76 0	30,06	38½	39	D.	{ ☉'s U. L. } easterly ☉'s L. L.
♂ 21	41 26									
	4 33	4	12 3	23 59 57	76 0	30,10	56	56	D.	{ ☉'s U. L. } ☉'s L. L.
	15 47	19 30	23 11		79 0	29,97	45	44	D.	{ ☉'s U. L. } ☉'s L. L.
	20 13	19	27 27							
	40 42	19 44	34 22		76 0	29,96	46	45	D.	{ ☉'s U. L. } easterly ☉'s L. L.
	45 16	49 7	53 54							
	47 20	4 44	8 41		73 40	29,95	47	47	D.	{ ☉'s U. L. } ☉'s L. L.
	5 27	9 38	13 25							
♂ 22	15 20	3	37 27		73 40	29,89	62	65	D.	{ ☉'s U. L. } ☉'s L. L.
	50 8	46 9	42 9							
	5 38	4 44	8 3		76 0	29,89	62½	65	D.	{ ☉'s U. L. } ☉'s L. L.
	10 2	6 18	2 22							
	30 36	26 52	5		79 0	29,89	63	65	D.	{ ☉'s U. L. } ☉'s L. L.
	34 57	31 18	57							

1768	Equal altitudes. Times by the clock.				Zenith distance	Baro- meter.	Thermo- meters.		Phænomena and Circum- stances.	
	Lower Wire.	Middle Wire	Upper Wire	Passed the Meridian			A	B	Ob- server.	
October.	' "	' "	' "	' "	o '	Inches				
♂ 26	10 48	2 14 6			78 0	29,64	19	15	W.	♂'s L. L.
	26 22:	20 53	15 20::							♂'s U. L.
	43 15	2 38 9	33 0:		80 0	29,63	18½	14½	W.	♂'s L. L.
	49 12	44 14	39 12							♂'s U. L.
☉ Nov. 6	57 45	8 1 15	4 42	13 19 27½	71 0	29,63	3	—4½	W.	Aldebaran easterly
	8 2	11 32	14 59		69 40					
		18 27 21	30 51		69 40	29,56	—3	—11	W.	Aldebaran westerly
	34 14	37 42	41 10		71 0					
♂ 16	37 55	8 41 35	45 12	12 44 3½	61 20	29,60	+9	—3	D.	Aldebaran easterly
	56 56	9 0 40	4 21		59 0					
	31 10	16 27 28	23 45		59 0	29,60	+1	—8	D.	Aldebaran westerly
	50 14	46 32			61 20					
♂ 17	24 1	7 27 27	30 56	12 40 34"	70 20	29,73	—1	—9	D.	Aldebaran easterly
	36 53	40 23:	43 52		68 40					
		53 15	56 46		67 0					
	31 21	17 27 50	24 24		67 0	29,83	—7	—12	D.	Aldebaran westerly
	44 15:	17 40 44	37 17		68 40					
	57 7	53 40:	50 13:		70 20					
♀ 18	1 52:	8 5 27	8 56	12 37 1,7	65 0	29,88	—14		D.	Aldeb. easterly, hazy
	12 11	7 8 38	5 6		65 0	29,80	—3	—15	D.	Aldebaran westerly
♂ 19	2 9	9 6 17	10 43		57 0	29,85	—9	—6	D.	Aldebaran easterly
	17 31	21 30	15 27		55 20				D.	Ditto Ditto
☉ 27	At noon I went to wind up the clock, but found it had stopped at 6 ^h 48'. I suppose it had been stopped by the cold last night, and therefore I kindled a fire to warm it before it was set a going. At about 6 ^h 58' by the alarm set the regulator a going and wound it up. J. D.									
♂ Dec. 6	13 28	6 16 58	20 24	11 22 11,1	69 20	29 94	—11½	—25½	W.	Aldebaran easterly
	26 19	29 49	33 18:		67 40					
	18 3	16 14 32			67 40	29 98	—16	—26	W.	Aldebaran westerly
	30 56	27 24			69 20					
♂ 8	6 22	6 9 52	13 19	11 15 6½	69 20	30 21	—2	—15	W.	Aldebaran easterly
	23 52	16 20 22	16 53		69 20	30 14	—4½	—14	W.	Aldebaran westerly.
♂ 10		6 10 21	13 49	11 7 57,3	68 20	29 58	—10	—23½	W.	Aldebaran easterly
	17 11	6 20 41	24 9		67 0					
	58 43½	15 55 13			67 0					
	8 4	16 5 34	2 6		68 20	29 51	—23	—32	W.	Aldebaran westerly.
☉ 11	3 23	6 6 55		11 4 29,8	68 20	29 50	—27	—37	D.	Aldebaran easterly.
	16 21	19 51	23 20		66 40					
		15 49 9			66 40					
	5 38	16 2 3			68 20	29 39	—31	—12	D.	Aldebaran westerly.

At 21^h I found that the regulator had stopped at 20^h 16' 3", notwithstanding the fire was very good, and by agreement with Mr. Wales, I let the fire go out, the stove being obliged to stand so near the side of the observatory that a little extraordinary fire would endanger the same, it having twice melted the lead at the back already; I also took off the weight off the regulator to ease it, and let it stand. J. D.

1769		Equal altitudes. Times by the clock.				Zenith distance	Baro- meter.	Thermo- meters.		Phænomena and Circum- stances.	
March.	Lower Wire " "	Middle Wire " "	Upper Wire " "	Passed the Meridian " "	" "	" "	Inches	A	B	Ob- server	
♂	13 39 1	7 43 50	18 39 10	7 37 $\frac{1}{2}$	53 0	29,76	-22	-30	W.	Regulus easterly	
	53 33	7 58 44	3 58		51 40						
	11 44				51 40	29,68	-26	-34	W.	Regulus westerly	
	36 13	12 31 25	26 57		53 0						
N.B. These were made by the assistant Clock.											
♂	21	Set Mr. Ellicott's clock a going.									
♂	25	32 48	9 36 25	39 59	13 36 26 $\frac{1}{4}$	57 20	29,81	+ 2	0	W.	Arcturus easterly
		13 29	47 7	50 44		57 0					
		29 22	17 25 43 $\frac{1}{2}$	22 7		56 0	29,80	-10	-17	W.	Arcturus westerly
		10 6	36 29	32 55		57 20					
♂	30	5 3	20 8 52	12 40		70 40	29,79	+ 1	+ 8	W.	♂'s U. L. } easterly
		9 37	13 28	17 16							♂'s L. L. }
♀	31	6 50	4 2 59	59 11	0 7 45,2	70 40	29,83	+14	+15	W.	♂'s L. L. } westerly
		11 25	7 35	3 40							♂'s U. L. }
♂ April	2	43 24	19 47 4	50 42		72 20	29,85	- 8	- 5	D.	♂'s U. L. } easterly
		47 45	51 25	55 5							♂'s L. L. }
♂	3	29 38	4 25 57	22 17	0 8 12,2	72 20	30,00	+ 5	+12	D.	♂'s U. L. } westerly
		14 0	30 20	26 40							♂'s L. L. }
♂	4	10 56	19 44 6	48 17		72 0	30,12	- 6	- 3	D.	♂'s U. L. } easterly
		45 17	48 58	52 37							♂'s L. L. }
♂	5	32 29	4 28 44	45 8		72 0	30,12	+12	+20	D.	♂'s L. L. } westerly
			33 15	29 35							♂'s U. L. }
		38 28	19 42 7	45 44		72 0	30,08	- 1	+ 8	D.	♂'s U. L. } easterly
		42 50		50 6							♂'s L. L. }
♂	6	35 16	4 31 36	28 0	0 8 33,4	72 0	30,14	+14	+16	D.	♂'s L. L. } westerly
		39 36	35 57	32 21							♂'s U. L. }
♂	8	20 28	19 24 2	27 35		73 20	30,00	+ 6	+12	W.	♂'s U. L. } easterly
		24 43	28 17	31 49 $\frac{1}{2}$							♂'s L. L. }
♂	9	53 58	4 50 23	46 51 $\frac{1}{2}$	0 8 50,8	73 20	29,99	+22	+27	W.	♂'s L. L. } westerly
		58 13	54 39	51 7 $\frac{1}{2}$							♂'s U. L. }
♂	10	26 16	19 29 51	33 24		72 0	30,20	- 7	- 6	W.	♂'s U. L. } easterly
		30 32	34 8	37 41							♂'s L. L. }
♂	11	48 41	4 45 5	41 31	0 9 7,6	72 0	30,24	+ 8	+12	W.	♂'s L. L. } westerly
		52 56 $\frac{1}{2}$	49 22	45 49							♂'s U. L. }
♂	12	13 47 $\frac{1}{2}$	19 17 21	20 13		73 0	29,73	+ 5	+12	W.	♂'s U. L. } easterly
		18 3	21 35	25 5							♂'s L. L. }
♂	14	1 44	4 58 10	54 41	0 9 23,7	73 0	29,63	+23	+17	W.	♂'s L. L. } westerly
		5 57	5 24	18 55							♂'s U. L. }
♂	15	I find from a mean of 8 comparisons made in the course of this week, and that which immediately preceded the last, that the assistant clock gains on Mr. Ellicott's at the rate of 3'', 03 in 6 hours: but from a mean of 4 taken the week preceding the last, it gained only 2'' 81; and from the 4 which were made this week, it gains 3'' 25 in six hours. W. W.									
		23 53	33 23	37 16		62 48	29,91	+ 1	+ 6	D.	♂'s U. L. } easterly
		14 7	33 3	41 57							♂'s L. L. }
♂	16	6 20	3 42 25	38 32	0 9 50,-	62 48	29 87	+13	+12	D.	♂'s L. L. } westerly
			47 5	43 12							♂'s U. L. }

1769	Equal altitudes. Times by the clock.				Zenith distance	Baro- meter	Thermo- meters		Phænomena and circum- stances	
	Lower Wire	Middle Wire	Upper Wire	Pasied the Meridian			Inches	A B		
April		h. m. s.	h. m. s.	h. m. s.						
h	22 14 6	20 17 48	21 32							
o	18 33	22 17	26 2		62 40	29,45	+ 22 + 3	W.	☉'s U. L.	easterly
	3 48	4 0 1		o 10 46,7	62 40	29,45	36 41	W.	☉'s L. L.	
	8 14	+ 4 31							☉'s U. L.	westerly
	3 12	19 6 41			71 20				☉'s U. L.	
	7 21	10 53	14 22						☉'s L. L.	easterly
		22 16 1/2	25 46						☉'s U. L.	flying clouds
	22 59	26 29	29 58		69 20	29,60	26 28	W.	☉'s U. L.	
	59 35	4 56 3	52 34	o 10 50,8	69 20	29,69	35 32	W.	☉'s L. L.	westerly a little uncertain because of clouds
	3 49	5 0 17	56 47						☉'s U. L.	
	15 14	11 43	8 13		71 20	29,69	34 31	W.	☉'s L. L.	easterly
	19 26	15 56	12 29						☉'s U. L.	thin cloud
	1 0	21	9 15		56 40	29,72	22 37	W.	☉'s L. L.	
	5 54	21 10 6	14 15						☉'s U. L.	
	10 48	3 12 37	8 30	o 11 1,5	56 40	29,75	36 45	W.	☉'s L. L.	westerly
	21 45	17 36	13 30						☉'s U. L.	
	25 12	19 28 43	32 13		68 0	29,67	27 33	W.	☉'s L. L.	easterly
	29 26	32 57	36 27						☉'s U. L.	
	53 39	4 50 8		o 11 7,5	68 0	29,59	43 45	W.	☉'s L. L.	westerly - very haz.
	57 52	54 23	50 50						☉'s U. L.	
	33 46	21 38 21	42 54		52 40	29,66	36 44	W.	☉'s L. L.	easterly
	39 17	43 54	18 28						☉'s U. L.	
	43 50	2 39 11	34 36	o 11 14,3	52 40	29,83	49 57	W.	☉'s L. L.	westerly
	49 22	44 47	10 12						☉'s U. L.	
	21 10	19 24 41	28 10		68 0	29,92	33 35	W.	☉'s L. L.	easterly
	25 23	28 55	32 25						☉'s U. L.	
	58 5	4 54 32	51 3	o 11 19,0	68 0	29,99	42 48	W.	☉'s L. L.	westerly
	2 17	58 45	55 15						☉'s U. L.	
May 3	5 26	20 9 5	12 42		61 0	30,20	32 38	D.	☉'s L. L.	easterly
	9 43	13 20	16 57						☉'s U. L.	
	15 43		8 26		61 0	30,20	45 49	D.	☉'s L. L.	westerly
	19 59	4 16 21	12 43	o 12 22 1/2					☉'s U. L.	
	3 34	20 7 10	10 47		61 0	30,16	39 47	D.	☉'s L. L.	easterly
	7 48	11 26	15 2						☉'s U. L.	
	17 37	4 13 58	10 22	o 12 22,8	61 0	30,03	55 62	D.	☉'s L. L.	westerly
	21 52	18 14	14 39						☉'s U. L.	
	39 32	19 43 6	46 37		62 40	29,97	24 22	W.	☉'s L. L.	easterly
	43 47	47 20	50 51						☉'s U. L.	
	44 53	4 41 22	37 53	o 14 3,05	62 40	30,12	27 25	W.	☉'s L. L.	westerly
	49 10	45 37	42 6						☉'s U. L.	
	59 56	5 7 36	15 46		53 20	30,13	27 25	W.		
	17 43	27 5	37 45		52 20	30,14	27 24	W.	☉'s U. L.	easterly
	53 49	7 44 29			52 20	30,15	23 21	W.		
	11 20	8 3 55	55 48		53 20	30,15	24 20	W.	☉'s L. L.	westerly
	47 13	8 52 7	57 6		44 40	30,14	23 17	W.		
	2 9	9 7 31	12 59	o 58 29,3	43 20	30,14	23 16	W.	Arcturus easterly	
	54 52	12 49 31	44 11		43 20	30,14	23 14	W.		
	9 44	13 4 49	59 50		44 40	30,14	19 14	W.	Arcturus westerly	

1769	Equal altitudes. Times by the clock.				Zenith distance	Baro- meter	Thermo- meters		Phænomena and Circum- stances.	
	Lower Wire	Middle Wire	Upper Wire	Passed the Meridian			A	B		
May	h m	h m	h m	h m s	o	Inches			Observer	
21	19 48	10 53 36	57 24		52 20	29.66	32	45	W.	☉'s U. L. East. unc.
	14 21	58 10	1 59							☉'s L. L. because of
22	11 19	3 37 29	33 40	0 17 38.3	52 20	29.75	40	46	W.	☉'s L. L. clouds
	5 50	42 3	38 16						W.	☉'s L. L. west. fly-
	At 20 ^h	put the clock back.							W.	☉'s U. L. ing clouds
	10 25	10 14 5	7 45							
	14 48	18 29	12 11	23 56 1.0	54 20	29.82	27	39	W.	☉'s U. L. easterly
23	17 48	3 33 56	0 15		54 20	29.89	44	48	W.	☉'s L. L. hazy
	11 59	38 18	34 39							☉'s L. L. westerly
24	14 7	10 17 48	11 31	23 56 43.7	53 40	29.81	34	46	W.	☉'s U. L. hazy
	18 31	22 13								☉'s U. L. east. very
25	5 18	3 31 35			53 40	29.70	47	59	W.	☉'s L. L. hazy
	9 42	36 0	32 18							☉'s L. L. west. fly-
26	18 33	9 2 3	5 30 ¹ / ₂	23 57 50.3	63 0	29.78	31	34	D.	☉'s U. L. ing clouds
	2 39	6 12 ¹ / ₂	9 3 ¹ / ₂							☉'s U. L.
	14 1	17 31	10 58		61 0					☉'s L. L. easterly
	8 8 ¹ / ₂	21 31	15 6							☉'s U. L.
27	17 57	+ 34 21 ¹ / ₂			61 0	29.85	47	49	D.	☉'s L. L. west. the
	12 31	33 35	35 6							☉'s U. L. almost
	13 23	49 50	16 25		63 0					☉'s L. L. covered
	17 28	53 59	50 32							☉'s U. L. with clo.
	17 56	9 1 25	4 52	23 58 12 ¹ / ₂	63 0	29.88	34	36	D.	☉'s U. L. at times
	2 3 ¹ / ₂	5 31 ¹ / ₂	8 59							☉'s L. L.
	13 25	16 54 ¹ / ₂	10 23		61 0	29.88	35	38	D.	☉'s L. L. easterly
	7 32 ¹ / ₂	21 3	24 31							☉'s U. L.
28	9 16	+ 35 44			61 0	29.90	49	49	D.	☉'s L. L. west. very
	13 21	39 51	36 35							☉'s U. L. hazy
	14 44	51 12 ¹ / ₂	47 49		63 0	29.89	49	49	D.	☉'s L. L. westerly
	18 51	55 21 ¹ / ₂	51 54 ¹ / ₂							☉'s U. L.
29 June	56 5	18 59 34 ¹ / ₂		23 59 47 ¹ / ₂	63 0	29.95	35	41	D.	☉'s L. L.
	0 11	9 3 39 ¹ / ₂	7 16							☉'s U. L.
	11 32	15 2	18 31		61 0	29.95	36	42	D.	☉'s L. L. easterly
	15 40	19 10 ¹ / ₂	12 37 ¹ / ₂							☉'s U. L.
	27 4	30 34 ¹ / ₂	4 4		59 0	29.95	36	43	D.	☉'s L. L.
	11 12 ¹ / ₂	34 43 ¹ / ₂	34 12 ¹ / ₂							☉'s U. L.
	12 40	46 13	49 44		57 0	29.95	37	45	D.	☉'s L. L.
	16 51 ¹ / ₂	50 25 ¹ / ₂	53 56							☉'s U. L.
30	12 58	4 9 25			57 0	29.89	52	58	D.	☉'s L. L.
		13 38	10 8							☉'s U. L.
	18 41 ¹ / ₂	25 10 ¹ / ₂	21 38		59 0	29.89	53	59	D.	☉'s L. L.
	12 47	29 17 ¹ / ₂	25 47 ¹ / ₂							☉'s U. L.
	14 11	40 41 ¹ / ₂	37 14		61 0	29.89	53	59	D.	☉'s L. L. westerly
	18 20	44 50	41 22							☉'s U. L.
	19 30 ¹ / ₂	56 11 ¹ / ₂	52 38 ¹ / ₂		63 0	29.89	53	60	D.	☉'s L. L.
	3 49	5 0 18								☉'s U. L.
	18 59	10 2 36			63 0	29.65	38	42	W.	☉'s U. L. easterly
	19 51	9 3 20	5 46							☉'s L. L.

1769	Equal altitudes. Times by the clock.				Zenith distance	Baro- meter	Thermo- meters		Phenomena and Circum- stances.	
	Lower Wire ' "	Middle Wire h ' "	Upper Wire ' "	Passed the Meridian h ' "			Inches	A B		
h	3 11 9	14 36	18 2		61 0	29,65	38 42	W.	☉'s U. L.	easterly very hazy
	15 18	18 45	22 12		59 0	29,65	39 45	W.	☉'s L. L.	
	26 37	19 30 7	33 35		57 0	29,64	40 46	W.	☉'s U. L.	
	30 47		37 48		57 0	29,64	40 46	W.	☉'s L. L.	
	42 14	45 47	49 18		57 0	29,64	40 46	W.	☉'s U. L.	
	46 29	50 2	53 32		57 0	29,55	52 54	W.	☉'s L. L.	
⊙	4 4 10 34::	7 1:	11 18::	0 0 9,3	57 0	29,55	52 54	W.	☉'s U. L.	westerly very un- certain because of clou.
		4 26 14	22 45		59 0	29,55	52 53	W.	☉'s L. L.	
	33 56::	30 28			61 0	29,55	51 52	W.	☉'s U. L.	
	45 17	41 49			60 0	29,47	37 43	W.	☉'s L. L.	
	49 27:	45 56::	42 33		58 0	29,47	38 43	W.	☉'s U. L.	
	18 36	19 22 6			56 0	29,47	38 44	W.	☉'s L. L.	
	22 46:		29 47		56 0	29,46	49 53	W.	☉'s U. L.	
	34 8	37 36	41 7		58 0	29,46	50 52	W.	☉'s L. L.	
	38 18	41 49	45 26		60 0	29,46	50 52	W.	☉'s U. L.	
	49 49	53 22	56 51		56 0	29,46	50 52	W.	☉'s L. L.	
	54 5	59 38	1 9		56 0	29,46	49 53	W.	☉'s U. L.	
	7 21	4 8 2	16 2::	0 0 34,8	58 0	29,46	50 52	W.	☉'s L. L.	
h	23 5	23 45::	20 17		60 0	29,46	50 52	W.	☉'s U. L.	westerly very cloudy and un- certain
	27 18	35 8	31 40		61 0	29,49	37 41	D.	☉'s L. L.	
	38 39	39 19			57 0	29,75	44 48	W.	☉'s U. L.	
	42 51	19 13 44			57 20	29,72	63 63	W.	☉'s L. L.	
	10 15 1/2	17 51	21 17 1/2		57 20	29,74	40 41	W.	☉'s U. L.	
	14 23	44 45 1/2	48 17		59 20	29,80	60 60	W.	☉'s L. L.	
⊙	41 16	48 59	52 30 1/2		57 0	29,66	55 54	D.	☉'s U. L.	westerly
	45 26	47 38	14 8 1/2	0 3 14,3	61 0	29,67	55 55	D.	☉'s L. L.	
	21 11	4 17 38	18 19		57 0	29,75	44 48	W.	☉'s U. L.	
	25 22 1/2	21 49 1/2	45 20		57 0	29,72	63 63	W.	☉'s L. L.	
	52 16	48 47			57 20	29,74	40 41	W.	☉'s U. L.	
	56 25 1/2	52 54 1/2			59 20	29,80	60 60	W.	☉'s L. L.	
h	19 25 14	19 28 44	32 12		59 20	29,75	44 48	W.	☉'s U. L.	easterly
	29 24	32 55	36 23		57 20	29,72	63 63	W.	☉'s L. L.	
	40 47	44 18	47 47 1/2		57 20	29,74	40 41	W.	☉'s U. L.	
	44 59	48 30	50 0 1/2		59 20	29,80	60 60	W.	☉'s L. L.	
	20 29 56	4 26 25	22 54	0 7 26 1/2	57 20	29,74	40 41	W.	☉'s U. L.	
	34 7	30 36	27 7		59 20	29,80	60 60	W.	☉'s L. L.	
h	45 27	41 57	38 28		57 20	29,74	40 41	W.	☉'s U. L.	easterly
	49 37	46 7	42 38		59 20	29,80	60 60	W.	☉'s L. L.	
	26 8 1/2	19 29 38	33 6		57 20	29,74	40 41	W.	☉'s U. L.	
	30 20	33 50	37 19		59 20	29,80	60 60	W.	☉'s L. L.	
	41 43 1/2	45 14	48 43		57 20	29,74	40 41	W.	☉'s U. L.	
	45 55	49 26	52 55 1/2		59 20	29,80	60 60	W.	☉'s L. L.	
h	22 30 45	4 27 14	23 43	0 8 19 3/4	57 20	29,74	40 41	W.	☉'s U. L.	westerly
	34 56	31 26	27 55		59 20	29,80	60 60	W.	☉'s L. L.	
	46 18	42 48	39 18		57 20	29,74	40 41	W.	☉'s U. L.	
	50 27	46 58	43 30		59 20	29,80	60 60	W.	☉'s L. L.	

1769		Equal altitudes. Times by the clock.				Z. with distance	Baro- meters	Thermo- meters		Phænomena and Circum- stances.	
June	Lower Wire	Middle Wire		Upper Wire	Passed the Meridian		Inches	A	B	Ob- server	
24	22 11 18	19 14 47	13 57	22 24 1/2		61 20	29,53	46	49	W.	☉'s U. L. easterly
♀	15 27 1/2	13 57	55 11	0 8 48,5		61 20	29,56	61	62	W.	☉'s L. L. westerly
	2 6	4 58 58	59 19								
	6 15	5 2 46									
	55 51	10 59 23				55 40	29,51	48	48	W.	☉'s U. L. easterly
	0 5	20	7 9								
24	18 18	4 14 45	11 13	0 9 12,8		55 40	29,60	57	56	W.	☉'s L. L. westerly
	22 32	18 59	15 27								
♂	27 3 49 1/2	20 7 20	10 56			55 0	29,69	50	59	D.	☉'s U. L. easterly
	8 1	11 34	15 9								
♀	28 13 49	4 10 14	6 41 1/2	0 10 58,6		55 0	29,80	66	65	D.	☉'s L. L. westerly
	18 0 1/2	14 29	10 56								
☉ July	23 59	19 27 28 1/2	30 56			60 40	29,57	52	56	W.	☉'s U. L. easterly
	28 8	31 38	35 5								
☽	3 57 55	4 54 26	50 58	0 13 8,0		60 40	29,49	70	79	W.	☉'s L. L. westerly
	2 4	58 34									
24	6 28 8 1/2	19 31 39	35 8			60 40					
	32 18	35 48 1/2	39 17								
	43 40	47 10 1/2	50 39 1/2			58 40	30,08	46	50	W.	☉'s U. L. easterly
	47 50 1/2	51 21	54 50								
♀	7 41 29	4 37 58	34 28	0 14 48,1		58 40					
	45 39	42 9	38 39								
	57 3		50 5			60 40	30,16	56	54	W.	☉'s L. L. westerly
	1 13	57 44	54 14								
24	13 49 44	19 53 16	56 46			59 0	29,73	48	48	D.	☉'s U. L. easterly
	53 54	57 26	0 57								
♀	14 40 38	4 37 8	33 37	0 17 27,5		59 0	29,68	55	57	D.	☉'s L. L. westerly
		41 17	37 47								
	56 20 1/2	19 59 53	3 24			53 20	29,60	49	48	D.	☉'s U. L. easterly
	0 31	20 4 4	7 35								
24	15 34 40	4 31 8	27 30 1/2	0 17 46,9		58 20	29,65	55	50	D.	☉'s L. L. westerly
	38 51	35 19 1/2	31 4								
24	27 55 57 1/2	19 59 29	3 0			61 0	29,61	51	54	D.	☉'s U. L. easterly
	0 7	20 3 40 1/2	7 12								
♀	28 41 56	4 38 24	34 53	0 21 18,9		61 0	29,67	67	70	D.	☉'s L. L. westerly
	46 6 1/2	42 35	39 4								
♂ Aug. 1	Put the clock back.									W.	
24	3 52 4	19 55 40	59 15			61 0	29,65	45	50	W.	☉'s U. L. easterly
	56 21 1/2	59 58	3 34								
♀	4 14 11	4 10 34 1/2	6 59	0 5 35,4		61 0	29,64	60	56 1/2	W.	☉'s L. L. westerly
	18 29	14 53	11 18								
☽	7 12 42 1/2	19 16 13	19 42 1/2			67 0	29,50	49	51	D.	☉'s U. L. easterly
	16 51 1/2	20 22 1/2									
♂	8 54 34	4 51 2 1/2		0 6 5 1/2		67 0	29,44	56	61	D.	☉'s L. L. westerly
	58 44	55 14	51 44								
♂	15 34 37 1/2	19 38 12	41 44 1/2			66 20	29,98	46	49	W.	☉'s U. L. easterly
	38 55	42 29									

Equal altitudes. Times by the clock.					Zenith distance	Baro- meter	Thermo- meters		Phænomena and Circum- stances.	
L.	Lower Wire	Middle Wire		Upper Wire	Passed the Meridian	Inches	A	B	Ob- server	
	h m s	h m s	h m s	h m s						
16	33 37	4 50 2	26 29	0 6 32.9	66 20	50.01	54	57	W.	<div> <div> O's L. L. </div> <div> O's U. L. </div> </div> } westerly
24	37 54	3 4 20	30 47		65 0	29.54	46	50	D.	<div> <div> O's U. L. </div> <div> O's L. L. </div> </div> } easterly
25	1 39	1 4 25	16 11	0 6 34.1	65 0	29.64	58	57	D.	<div> <div> O's L. L. </div> <div> O's U. L. </div> </div> } westerly

Apparent Times.			Zenith distances.				Baro- meter.	Thermo- meters.		Phænomena and Circum- stances.							
r	h	m	o	1	2	3	4	Inches	A	B	Ob- server						
5			55	52	33	59	2	20	55	51	49	29.61	46	42	W.	⊙'s U. L. on merid.	
10			48	20	58	62	0	30	9	58	20	32	30.26	47	41	D.	⊙'s L. L. ditto.
11			58	12	30	62	0	11	6	58	12	15	30.09	45	40	D.	⊙'s U. L. ditto.
12			59	7	44	63	0	9	16	59	7	26	29.93	57	60	D.	⊙'s L. L. ditto.
17			60	32	24	64	2	10	21	60	32	10	29.77	47	48	W.	⊙'s U. L. ditto.
19			50	30	56	53	3	15	6	50	29	56	29.99	52	49	W.	α aquilæ on the merid.
			9	46	57	10	1	24	2	9	47	2	29.97	43	38	W.	α persei do. pl. qua. E.
15 40 56			53	19	56	37	2	22	10	35	18	53	29.78	42	38	W.	⊙'s U. L. on merid.
0			61	42	40	65	3	10	25	61	42	25					⊙'s U. L. }
			62	15	0	66	1	19	13	62	14	42	29.98	47	47	W.	⊙'s L. L. } on merid.
1			62	38	15	66	3	9	15	62	38	23	29.79	46	45	W.	⊙'s L. L. on merid.
2			14	19	21	15	1	3	16	14	18	52	29.19	46	39	D.	α cygni ditto.
3			50	30	34	53	3	15	4	50	29	58	29.82	32	28	W.	α aquilæ ditto. (v. g.)
			14	19	25	15	1	3	0	14	19	8	29.82	32	28	W.	α cygni ditto. (v. g.)
			13	2	44	13	3	22	12	13	2	54	29.84	28	27	W.	capella do. pl. qu. W.
4			67	2	54	71	2	2	2	67	2	43					⊙'s U. L. }
			67	35	25	72	0	12	10	67	35	6	29.90	37	34	W.	⊙'s L. L. } merid.
			50	30	49	53	3	16	8	50	30	20	29.90	38	32	W.	α aquilæ ditto.
			9	45	52	10	1	21	0	9	45	47	29.86	31	28	W.	α persei do. pl. qu. W.
			13	3	12	13	3	23	25	13	3	8	29.83	31	27	W.	capell. do. do. ver. hazy
5			68	19	22	72	3	16	17	68	18	56	30.20	25	18	D.	⊙'s L. L. on merid.
			20	12	18	21	2	7	6	20	12	21	30.18	27	21	D.	α lyrae ditto.
			50	30	52	53	3	16	6	50	30	22	30.18	27	21	D.	α aquilæ ditto.
			14	19	30	15	1	4	11	14	19	23	30.17	26	20	D.	α cygni ditto.
			68	9	20	72	2	25	16	68	8	51	30.22	29	23	D.	⊙'s U. L. ditto.
			20	12	20	21	2	7	2	20	12	25	30.18	29	26	D.	α lyrae ditto.
			14	19	36	15	1	4	6	14	19	28	30.14	28	25	D.	α cygni ditto.
			9	46	48	10	1	23	16	9	46	24	30.05	25	21	D.	α persei do. pl. qu. E.
			13	3	34	13	3	23	20	13	3	13	30.01	27	24	D.	capella ditto. ditto.
			50	30	24	53	3	15	14	50	29	48	29.87	19	11	W.	α aquila on meridian
			14	18	51	15	1	3	18	14	18	50	29.87	17	10	W.	α cygni ditto.
			9	45	54	10	1	21	6	9	45	41	30.07	14	9	W.	α per. do. pl. qu. E. haz.

1768	Apparent Times	Zenith distances.					Barometer.	Thermometers.		Phænomena and Circumstances.		
November	h	m	s	90 Arch	96 Arch	ub.	96 Arch reduced	Inches	A	B	Observer	
				o	"	G. S. V.	"					
6				50 29 11	53 3 12	6	50 28 37	29,65	+ 6	-3	W.	α aquilæ on meridian
				14 18 40	15 1 2	3	14 18 38	29,65	+ 5	-3½	W.	α cygni ditto.
9				50 29 38	53 3 14	12	50 29 23	29,89	0	-3½	W.	α aquilæ ditto.
16				14 18 48	15 1 2	0	14 18 41	29,58	+ 4	-5	D.	α cygni ditto.
* * Many of the preceding observations can be of no use in determining the latitude of the place; but I thought it might be useful to infer them, as they serve to shew what a very great alteration happened in the position of the line of collimation of the quadrant, about this time. W. W.												
16				9 45 40	10 1 20	15	9 45 6	29,62	+ 7	-2	D.	α per. on m. pla. qu. E.
				13 2 53	13 3 21	20	13 2 20	29,63	+ 5	-2	D.	capella ditto ditto.
18	5	32	43	65 0 0								
		37	30	64 40 0								
		42	31	64 20				29,87	-6½	-12½	D.	D's L. L. east merid.
		47	50	64 0 0								
		53	11	63 40 0								
	7	28	28	60 11 16	64 2 7	14		29,87	-8	-13	D.	D's L. L. on merid.
		36	28	60 30 20								
		41	28	60 30 50				29,88	-8	-14	D.	D's L. L. west merid.
		49	8	60 33 16								
		54	28	60 36 48								
				9 46 32	10 1 23	20	9 46 20	29,88	-2	-13½	D.	α perfei } on mer. pl.
				13 3 32	13 3 23	24	13 3 9	29,89	-1	-14	D.	capella } of qu. W.
				51 25 44	54 3 13	0	51 25 24	29,90	0	-14	D.	α orionis on merid.
19				9 46 0	10 1 21	0	9 45 47	29,85	-10	-15	D.	α perfei do. pl. qu. E.
28				9 45 48	10 1 20	4	9 45 17	29,35	-8	-18	D.	α perfei } on merid. pl.
				13 2 26	13 3 20	4	13 2 10	29,37	-10	-18½	D.	capella } of quad. E.
29				9 47 30	10 1 24	3	9 47 3	29,51	-5	-9	D.	α perfei } on merid. pl.
				13 3 28	13 3 23	3	13 3 30	29,61	-4	-12	D.	capella } of quad. W.
Dec. 2				58 11 18	62 0 9	24	58 11 3	29,55	-5	-16	D.	α urf. maj. on m. bel. p.
				29 16 20	31 0 28	12	29 15 51	29,53	-6	-16	D.	polaris do. above pole
				65 2 10	69 1 15	16	65 1 38	29,52	-6	-16	D.	ζ urf. maj. do. bel. pole
3				58 11 30	62 0 9	13	58 11 14	29,56	-1	-8	D.	α urf. maj. do. do.
				29 16 17	31 0 28	12	29 15 51	29,56	-2	-10	D.	polaris do. above pole
				65 2 7	69 1 15	24	65 1 30	29,56	-2½	-10½	D.	ζ urf. maj. do. bel. pole
				9 46 14	10 1 21	16	9 45 32	29,53	-3	-12	D.	α perfei do. pl. qu. E.
6				58 11 32	62 0 9	19	58 11 8	29,54	-11½	-23	W.	α urf. maj. do. bel. pole
				9 47 30	10 1 24	0	9 47 7	29,95	9	-2	W.	α perfei do. pl. qu. W.
7				66 11 48	70 2 13	0	66 11 20	30,10	-10	-21½	W.	γ urf. maj. do. bel. pole
24				66 11 40	70 2 13	0	66 11 20	30,21	-3	-15½	W.	do. do. do.
				65 2 41	69 1 16	4	65 2 17	30,21	-3	-16	W.	ζ urf. maj. do. do.
10				9 46 0	10 1 21	12	9 45 35	29,54	-10	-25	W.	α perfei do. pl. qu. E.
15				58 11 20	62 0 8	0	58 11 1	29,92	-24	-25	D.	α urf. maj. do. bel. pole
				29 15 56	31 0 28	16	29 15 47	29,94	-23½	-25	D.	polaris do. above pole
				65 1 57	69 0 14	8		29,95	-23½	-25½	D.	ζ urf. maj. do. bel. pole
19				66 11 39	70 2 13	4	66 11 12	29,89	-15	-21	W.	γ urf. maj. do. do.
				29 16 16	31 0 28	14	29 15 49	29,89	-15	-21	W.	polaris do. above pole

1769	Apparent Times	Zenith distances.				Barometer	Thermometers		Phænomena and Circumstances.	
January		90 Arch.	96 Arch.	Su.	96 Arch reduced	Inches	A	B	Ob-	server
		° ' "	G. S. V.	"	° ' "					
☉	1	70 41	4 75 1 19	11	70 40 59	29.94	-25	-29	W.	☿ urf. maj. on mer. bel. the pole
☽	2	33 6 28	35 1 8	0	33 6 20	30.16	-25	-27	W.	polaris, ditto, ditto
☿	17	70 41	9 75 1 19	20	70 40 50	30.19	-28	-34	W.	☿ urf. maj. ditto, ditto
♂	18	42 45 20	45 2 13	0	42 45 5	29.46	-34	-39	W.	aldebaran on the meridian
		29 16 16	31 0 28	12	29 15 49	29.45	-34	-38	W.	polaris on merid. above the pole
		65 2 20	69 1 15	20	65 1 34	29.45	-34	-39	W.	☿ urf. maj. ditto below the pole
		70 41	30 75 1 19	14	70 40 56	29.44	-34	-39	W.	☿ urf. maj. ditto, ditto
		42 45 20	45 2 13	20	42 45 12	29.36	-34	-36	W.	aldebaran on the meridian
♀	20	65 2 27	69 1 15	14	65 1 40	29.45	-27	-30	W.	☿ urf. maj. on merid. bel. the pole
		2 39 44	2 3 12	17	2 39 46	29.55	-33	-36	W.	ditto, ditto above the pole
		8 20 20	8 3 19	16	8 20 16	29.55	-33	-36	W.	☿ ditto, ditto, ditto
♂	21	70 41	28 75 1 19	15	70 40 55	29.74	-31	-36	W.	ditto, ditto below the pole
♂	28	75 24 10	80 1 22	2	75 23 42	30.13	-37	-40	W.	capella, ditto, ditto
☉	29	70 41	38 75 1 19	8	70 41 2	30.08	-30	-34	W.	☿ urf. maj. ditto, ditto
		9 46 7	10 1 21	3	9 45 44	30.07	-30	-35	W.	☿ urf. maj. ditto, ditto
		13 3 7	13 3 22	0	13 3 6	30.05	-31	-36	W.	☿ urf. maj. ditto, ditto
		2 39 52	2 3 12	14	2 39 44	30.00	-35	-40	W.	☿ urf. maj. ditto, ditto
		8 19 4	8 3 16	10	8 19 3	30.00	-35	-40	W.	☿ urf. maj. ditto, ditto
		72 7 52	76 3 24	24	72 7 30	29.99	-36	-41	W.	☿ urf. maj. ditto, ditto
		75 23 56	80 1 22	16	75 23 28	29.98	-36	-41	W.	☿ urf. maj. ditto, ditto
☽	30	70 41	28 75 1 19	15	70 40 55	29.97	-28	-31	W.	☿ urf. maj. ditto, ditto (hazy)
♂	31	13 3 8	13 3 22	10	13 2 56	29.96	-29	-30	W.	capella, ditto above the pole
		9 40 16	10 1 22	20	9 45 54	30.17	-24	-31	W.	☿ urf. maj. ditto, ditto
☉ Feb.	5	13 3 10	13 3 22	13	13 2 54	30.16	-26	-31	W.	capella, ditto, ditto
		13 3 16	13 3 23	20	13 3 13	29.79	-16	-20	D.	☿ urf. maj. ditto, ditto
		51 26 36	54 3 16	24	51 26 19	29.79	-16	-20	D.	☿ urf. maj. ditto, ditto
♂	8	42 45 30	45 2 13	12	42 44 53	30.03	-13	-12	D.	☿ urf. maj. ditto, ditto
		13 3 30	13 3 23	10	13 3 23	30.03	-13	-13	D.	☿ urf. maj. ditto, ditto
		51 26 32	54 3 16	24	51 26 19	30.02	-13	-13	D.	☿ urf. maj. ditto, ditto
♀	9	33 7 16	35 1 10	20	33 6 52	29.83	-13	-9	D.	☿ urf. maj. ditto, ditto
		42 45 38	45 2 13	10	42 44 55	29.60	+7	+12	D.	☿ urf. maj. ditto, ditto
☽	10	13 3 28	13 3 23	24	13 3 19	29.59	+9	+12	D.	☿ urf. maj. ditto, ditto
		33 7 36	35 1 10	10	33 7 23	30.32	-16	-21	D.	☿ urf. maj. ditto, ditto
☽	13	75 24 17	80 1 23	20	75 23 50	30.33	-21	-26	D.	☿ urf. maj. ditto, ditto
♂	14	75 24 22	80 1 23	18	75 23 52	29.93	-26	-27	W.	☿ urf. maj. ditto, ditto
		9 46 26	10 1 22	10	9 46 4	29.98	-17	-21	W.	☿ urf. maj. ditto, ditto
		13 3 28	13 3 22	6	13 3 0	29.96	-18	-21	W.	☿ urf. maj. ditto, ditto
♀	22	42 45 52	45 2 14	0	42 45 32	29.74	-19	-29	D.	☿ urf. maj. ditto, ditto
		51 26 20	54 3 15	22	51 25 55	29.72	-21	-30	D.	☿ urf. maj. ditto, ditto
☽	24	13 3 28	13 3 23	24	13 3 19	29.83	-20	-26	D.	☿ urf. maj. ditto, ditto
		51 26 24	54 3 15	20	51 25 57	29.84	-20	-28	D.	☿ urf. maj. ditto, ditto
♂	25	33 7 17	35 1 10	16	33 6 56	29.93	-29	-37	D.	☿ urf. maj. ditto, ditto
♀	26	51 26 4	54 3 14	14	51 25 36	29.90	-25	-31	D.	☿ urf. maj. ditto, ditto
☽	27	42 45 24	45 2 13	3	42 45 2	30.02	-28	-31	W.	☿ urf. maj. ditto, ditto
♂	3	42 45 22	45 2 13	0	42 45 5	29.86	-18	-15	W.	☿ urf. maj. ditto, ditto
♀	6	33 7 20	35 1 10	22	33 6 50	29.70	-31	-36	D.	☿ urf. maj. ditto, ditto
		75 24 34	80 1 24	22	75 24 14	29.69	-36	-41	D.	☿ urf. maj. ditto, ditto

1769		Appar. Time	Zenith distances.						Baro- meter	Thermo- meters		Phænomena and Circumstances.					
May			50 Arch		56 Arch		Subst.	56 Arch reduced	Inches	A	B	(Ob- server					
			h	i	h	i	G. S. V.	h	i								
♀	1	5	57	22	50	57	0	30, 14	+27	+24	W.	D's U. L. east of the merid					
		0	0	31	54	0											
		4	10	51	0												
		6	59	47	0												
		21	5	50	44	52											
		36	51	42	0	54	0						15	4	30	44	1 1/2
		39	15	51	0												
		42	58	54	0												
		46	12	57	0												
		51	54	51	3	0											
♂	2	5	57	22	50	57	0	30, 14	+21	+15	W.	arcturus on the meridian					
		0	0	31	54	0											
		4	10	51	0												
		6	59	47	0												
		21	5	50	44	52											
		36	51	42	0	54	0						15	4	30	44	1 1/2
		39	15	51	0												
		42	58	54	0												
		46	12	57	0												
		51	54	51	3	0											
♂	2	5	57	22	50	57	0	30, 14	+21	+15	W.	arcturus on the meridian					
		0	0	31	54	0											
		4	10	51	0												
		6	59	47	0												
		21	5	50	44	52											
		36	51	42	0	54	0						15	4	30	44	1 1/2
		39	15	51	0												
		42	58	54	0												
		46	12	57	0												
		51	54	51	3	0											
♂	2	5	57	22	50	57	0	30, 14	+21	+15	W.	arcturus on the meridian					
		0	0	31	54	0											
		4	10	51	0												
		6	59	47	0												
		21	5	50	44	52											
		36	51	42	0	54	0						15	4	30	44	1 1/2
		39	15	51	0												
		42	58	54	0												
		46	12	57	0												
		51	54	51	3	0											
♂	2	5	57	22	50	57	0	30, 14	+21	+15	W.	arcturus on the meridian					
		0	0	31	54	0											
		4	10	51	0												
		6	59	47	0												
		21	5	50	44	52											
		36	51	42	0	54	0						15	4	30	44	1 1/2
		39	15	51	0												
		42	58	54	0												
		46	12	57	0												
		51	54	51	3	0											
♂	2	5	57	22	50	57	0	30, 14	+21	+15	W.	arcturus on the meridian					
		0	0	31	54	0											
		4	10	51	0												
		6	59	47	0												
		21	5	50	44	52											
		36	51	42	0	54	0						15	4	30	44	1 1/2
		39	15	51	0												
		42	58	54	0												
		46	12	57	0												
		51	54	51	3	0											
♂	2	5	57	22	50	57	0	30, 14	+21	+15	W.	arcturus on the meridian					
		0	0	31	54	0											
		4	10	51	0												
		6	59	47	0												
		21	5	50	44	52											
		36	51	42	0	54	0						15	4	30	44	1 1/2
		39	15	51	0												
		42	58	54	0												
		46	12	57	0												
		51	54	51	3	0											
♂	2	5	57	22	50	57	0	30, 14	+21	+15	W.	arcturus on the meridian					
		0	0	31	54	0											
		4	10	51	0												
		6	59	47	0												
		21	5	50	44	52											
		36	51	42	0	54	0						15	4	30	44	1 1/2
		39	15	51	0												
		42	58	54	0												
		46	12	57	0												
		51	54	51	3	0											
♂	2	5	57	22	50	57	0	30, 14	+21	+15	W.	arcturus on the meridian					
		0	0	31	54	0											
		4	10	51	0												
		6	59	47	0												
		21	5	50	44	52											
		36	51	42	0	54	0						15	4	30	44	1 1/2
		39	15	51	0												
		42	58	54	0												
		46	12	57	0												
		51	54	51	3	0											
♂	2	5	57	22	50	57	0	30, 14	+21	+15	W.	arcturus on the meridian					
		0	0	31	54	0											
		4	10	51	0												
		6	59	47	0												
		21	5	50	44	52											
		36	51	42	0	54	0						15	4	30	44	1 1/2
		39	15	51	0												
		42	58	54	0												
		46	12	57	0												
		51	54	51	3	0											
♂	2	5	57	22	50	57	0	30, 14	+21	+15	W.	arcturus on the meridian					
		0	0	31	54	0											
		4	10	51	0												
		6	59	47	0												
		21	5	50	44	52											
		36	51	42	0	54	0						15	4	30	44	1 1/2
		39	15	51	0												
		42	58	54	0												
		46	12	57	0												
		51	54	51	3	0											
♂	2	5	57	22	50	57	0	30, 14	+21	+15	W.	arcturus on the meridian					
		0	0	31	54	0											
		4	10	51	0												
		6	59	47	0												
		21	5	50	44	52											
		36	51	42	0	54	0						15	4	30	44	1 1/2
		39	15	51	0												
		42	58	54	0												
		46	12	57	0												
		51	54	51	3	0											
♂	2	5	57	22	50	57	0	30, 14	+21	+15	W.	arcturus on the meridian					
		0	0	31	54	0											
		4	10	51	0												
		6	59	47	0												
		21	5	50	44	52											
		36	51	42	0	54	0						15	4	30	44	1 1/2
		39	15	51	0												
		42	58	54	0												
		46	12	57	0												
		51	54	51	3	0											
♂	2	5	57	22	50	57	0	30, 14	+21	+15	W.	arcturus on the meridian					
		0	0	31	54	0											
		4	10	51	0												
		6	59	47	0												
		21	5	50	44	52											
		36	51	42	0	54	0						15	4	30	44	1 1/2
		39	15	51	0												
		42	58	54	0												
		46	12	57	0												
		51	54	51	3	0											
♂	2	5	57	22	50	57	0	30, 14	+21	+15	W.	arcturus on the meridian					
		0	0	31	54	0											
		4	10	51	0												
		6	59	47	0												
		21	5	50	44	52											
		36	51	42	0	54	0						15	4	30	44	1 1/2
		39	15	51	0												
		42	58	54	0												
		46	12	57	0												
		51	54	51	3	0											
♂	2	5	57	22	50	57	0	30, 14	+21	+15	W.	arcturus on the meridian					
		0	0	31	54	0											
		4	10	51	0												
		6	59	47	0												
		21	5	50	44	52											
		36	51	42	0	54	0						15	4	30	44	1 1/2
		39	15	51	0												
		42	58	54	0												
		46	12	57	0												
		51	54	51	3	0											
♂	2	5	57	22	50	57	0	30, 14	+21	+15	W.	arcturus on the meridian					
		0	0	31	54	0											
		4	10	51	0												
		6	59	47	0												
		21	5	50	44	52											
		36	51	42	0	54	0						15	4	30	44	1 1/2
		39	15	51	0												
		42	58	54	0												
		46	12	57	0												
		51	54	51	3	0											
♂	2	5	57	22	50	57	0	30, 14	+21	+15	W.	arcturus on the meridian					
		0	0	31	54	0											
		4	10	51	0												
		6	59	47	0												
		21	5	50	44	52											
		36	51	42	0	54	0						15	4	30	44	1 1/2
		39	15	51	0												
		42	58	54	0												
		46	12	57	0												
		51	54	51	3	0											
♂	2	5	57	22	50	57	0	30, 14	+21	+15	W.	arcturus on the meridian					
		0	0	31	54	0											
		4	10	51	0												
		6	59	47	0												
		21	5	50	44	52											
		36	51	42	0	54	0						15	4	30	44	1 1/2
		39	15	51	0												
		42	58	54	0												
		46	12	57	0												
		51	54	51	3	0											
♂	2	5	57	22	50	57	0	30, 14	+21	+15	W.	arcturus on the meridian					
		0	0	31	54	0											
		4	10	51	0												
		6	59	47	0												
		21	5	50	44	52											
		36	51	42	0	54	0						15	4	30	44	1 1/2
		39	15	51	0												
		42	58	54	0												
		46	12	57	0												
		51	54	51	3	0											
♂	2	5	57	22	50	57	0	30, 14	+21	+15	W.	arcturus on the meridian					
		0	0	31	54	0											
		4	10	51	0												
		6	59	47	0												
		21	5	50	44	52											
		36	51	42	0	54	0						15	4	30	44	1 1/2
		39	15	51	0												
		42	58	54	0												
		46	12	57	0												
		51	54	51	3	0											
♂	2	5	57	22	50	57	0	30, 14	+21	+15	W.	arcturus on the meridian					
		0	0	31	54	0											
		4	10	51	0												
		6	59	47	0												
		21	5	50	44	52											
		36	51	42	0	54	0						15	4	30	44	1 1/2
		39	15	51	0												
		42	58	54	0												
		46	12	57	0												
		51	54	51	3	0											
♂	2	5	57	22	50	57	0	30, 14	+21	+15	W.	arcturus on the meridian					
		0	0	31	54	0											
		4	10	51	0												
		6	59	47	0												
		21	5	50	44	52											
		36	51	42	0	54	0						15	4	30	44	1 1/2
		39	15	51	0												
		42	58	54	0												
		46	12	57	0												
		51	54	51	3	0											
♂	2	5	57	22	50	57	0	30, 14	+21	+15	W.	arcturus on the meridian					
		0	0	31	54	0											
		4	10	51	0												
		6	59	47	0												
		21	5	50	44	52											
		36	51	42	0	54	0						15	4	30	44	1 1/2
		39	15	51	0												
		42	58	54	0												
		46	12	57	0												
		51	54	51	3	0											
♂	2	5	57	22	50	57	0	30, 14	+21	+15	W.	arcturus on the meridian					
		0	0	31	54												

1769		Apparent Times		Zenith distances.						Barometer	Thermometers		Phænomena and Circumstances.						
August		h	m	90 Arch		96 Arch		S.	96 Arch reduced		Inches	A	B	Ob-	server				
				°	'	"	G. S.	V.	"	°	'	"							
h	5			42	15	36	45	0	9	0	42	15	12	29,67	55	54	W. } ☉'s L. L. } W. } ☉'s U. L. } on the merid.		
				41	43	46	44	2	2	23	41	43	37						
		2	37	1	35	14	21	69	2	11	21	65	13	52					
			42	6	55	11	53	69	2	6	25	11	36						
			46	29	65	10	50	2	3	18	10	24							
			54	17	55	10	0	2	1	10	9	39	29,70	59	57	W. } ☉'s U. L.			
			58	59	55	10	34	2	2	16	9	57							
			3	4	4	55	12	20	2	6	6	11	55						
				9	15	65	14	22	2	10	0	65	13	46					
						55	21	4	69	2	25	1	0	65	20	22	29,79	51	47
				00	12	36	64	0	28	18	60	12	0	29,80	51	47			
h	12			44	14	50	47	0	25	15	44	14	29	29,63	56	61	D. } ☉'s L. L. on the merid.		
		20	14	20	28	48	30	0											
				23	14	48	35	0						29,81	44	44	D. } ☉'s L. L. east of merid.		
				28	43	48	31	0											
				20	33	48	27	0						29,81	45	44	D. Ditto on the meridian		
				31	40	48	23	0											
						46	5	42	51	1	6	18	48	5	9	29,81	45	43	D. Ditto west of the merid.
		15	29	2	48	23	0												
				32	26	48	27	0							29,81	45	43	D. Ditto west of the merid.	
				35	19	48	31	0											
h	22			37	53	48	35	0						29,48	54	59	☉'s L. L. on the merid.		
				40	51	48	40	0											

				Occultations of Fixed Stars by the Moon, &c. Observed.				
1768	Time per clock		Apparent Time					
September	h	m	s	h	m	s		
8	21	7	2	9	7	6	52	{ V immersed behind the ☽'s dark limb { J. D. W. W.
		7	2	10	7	6	59	
1769								
8 Mar. 15	11	21	6	11	24	34	{ ζ immersed behind the ☽'s dark limb (very exact) W. W. Ditto emerged (perhaps about 5" sooner) J. D.	
		12	8	44	12	12		11
8	29	16	54	0	16	46	24, ♀ immersed behind the bright limb of the moon { W. W. J. D.	
		53	58	16	46	19		
☉ Apr. 9	10	29	21	10	20	27½	{ τ immersed behind the Moon's dark limb J. D.	
		15	38	44	15	29		39
☽	10	15	38	44	15	30	{ 14's 1st satellite immersed close to the body of the planet { W. W. J. D.	
		39	14	15	30	9		
☉ Aug. 11	9	16	47	9	10	22½	{ The * N° 43 of Ophiuchi in Mr. Flamsteed's catalogue immersed { J. D. behind the dark limb of the ☽ (very faint)	
		10	14	56	10	8		31
	10	14	54	10	8	29	{ B in the same constellation and catalogue immersed J. D. Ditto per W. W. N. B. The immersion happened towards the northern limb of the ☽ so very near the intersection of light and darkness, as to render the observation doubtful to 2 or 3".	
		10	14	54	10	8		29

B in the same constellation and catalogue immersed J. D.
Ditto per W. W. N. B. The immersion happened towards the northern limb of the ☉ so very near the intersection of light and darkness, as to render the observation doubtful to 2 or 3".

The following Table for the MICROMETER I received from
the late Mr. Short, along with the Instrument.
Wm. Wales.

Inches	"	Decim. of an in.	"	Vern	"	Vern	"
1	6 50,2	0,05	0 20,5	1	0,8	20	16,4
2	13 40,4	10	0 41,0	2	1,6	21	17,2
3	20 30,6	15	1 1,5	3	2,5	22	18,1
4	27 20,9	20	1 22,0	4	3,3	23	18,9
5	34 11,1	25	1 42,6	5	4,1	24	19 7
		30	2 3,1	6	4,9		
		35	2 23,6	7	5,7		
		40	2 44,1	8	6,6		
		45	3 4,6	9	7,4		
		50	3 25,1	10	8,2		
		55	3 45,6	11	9,0		
		60	4 6,1	12	9,8		
		65	4 26,6	13	10,7		
		70	4 47,2	14	11,5		
		75	5 7,7	15	12,3		
		80	5 28,2	16	13,1		
		85	5 48,7	17	13,9		
		90	6 9,2	18	14,8		
		95	6 29,7	19	15,6		

1769	Times per clock			Apparent Times		Parts of the micrometer		Micro-meter reduced		Observations on the Transit of Venus.	
June	h	'	"	h	'	"	In-ches	Vernier	'	"	
½ 30	56	49	0	57	0	6					Exterior contact at the ingrefs
	56	56	0	57	7	6					Ditto
1	15	10	1	15	21	3					Interior ditto
	15	14	1	15	25	3					Ditto
	57	21		57	31	½	0,40	18	2	57,5	Diff. of ♀'s farthest limb from the ☉'s nearest
	58	36		58	46	½	0,10	22	0	57,5	♀'s diameter off the scale
2	1	16	2	1	26	½	4,60	4½	31	32,3	☉'s diameter
	2										Cloudy a short time
	4	11		4	21	½	4,25	10	29	13,3	Diff. of Venus's farthest limb from the ☉'s farthest
	5	58		6	8	½	0,10	19	0	58,2	♀'s diameter on the scale
	7	33		7	43	½	0,50	0½	3	27,1	Diff. of ♀'s farthest limb from the ☉'s nearest
	9	9		9	19	½	0,10	19	0	58,2	♀'s diameter on the scale
	10	26		10	36	½	0,15	0	0	59,9	Ditto off the scale
	12	0									Cloudy

J. D.
W. W.
W. W.
J. D.

J. D.

Observations on the Transit of Venus.

1769	Times per clock			Apparent times			Parts of the micrometer			Micro- meter reduced			
June	h	'	"	h	'	"	In- ches	Ver- nier	'	"			
h 3	2	39	0	2	39	10	4,60	1 $\frac{1}{2}$	31	29,8	} ☉'s horizontal diameter		
							4,60	1	31	29,4			
		44	43		44	52 $\frac{3}{4}$	0,70	7 $\frac{1}{2}$	4	54,9	} Dist. of ♀'s farthest limb from the ☉'s nearest		
		51	40		51	49 $\frac{1}{2}$	0,10	20 $\frac{1}{2}$	0	59,4			
		53	26		53	35 $\frac{1}{2}$	0,15	2	1	1,5	} Ditto off		
		3 4	58	3	5	7 $\frac{1}{2}$	0,80	4	5	33,1			
		6	13		6	22 $\frac{1}{2}$	0,10	21	0	59,8	} Dist. of ♀'s farthest limb from the ☉'s nearest		
											} ☉'s diameter on the scale		
		17	47		17	56 $\frac{1}{4}$	4,60	4	31	31,9	} Cloudy		
		19	40		19	49 $\frac{1}{4}$	0,85	6 $\frac{1}{2}$	5	55,2			
		22	20		22	29 $\frac{1}{2}$	0,85	14 $\frac{1}{2}$	6	1,8	} ☉'s inclined diameter		
		23	38		23	47	0,10	21	0	59,8			
		23	28		23	47	0,10	21	0	59,8	} Dist. of ♀'s farthest limb from the ☉'s nearest		
		24	35		24	44	0,10	23 $\frac{1}{2}$	0	53,7			
		42	47		42	55 $\frac{1}{4}$	0,10	24	0	59,1	} Ditto off		
											} Ditto ditto		
											} N. B. Several of the above observations are a little un- certain, being taken in great haste, in the intervals be- tween flying clouds. W. W.		
		46	40		46	48 $\frac{1}{2}$	0,90	10 $\frac{1}{2}$	6	19,4			
		48	0								} Dist. of ♀'s farthest limb from the ☉'s nearest		
		48	49		48	57 $\frac{1}{2}$	0,90	11	6	19,8			
		51	33		51	41 $\frac{1}{2}$	0,90	12	6	20,6	} A small cloud		
		55	24		55	32 $\frac{1}{2}$	0,90	12	6	20,6			
		56	19		56	27 $\frac{1}{2}$	0,90	12 $\frac{1}{2}$	6	21,1	} Dist. of ♀'s farthest limb from the Sun's nearest		
		59	2		59	10 $\frac{1}{2}$	0,90	12 $\frac{1}{2}$	6	20,6			
		+ 0	50	+ 0	58 $\frac{1}{2}$		0,90	11 $\frac{1}{2}$	6	20,2	} Ditto		
		2	51		2	59 $\frac{1}{2}$	0,90	11 $\frac{1}{2}$	6	20,2			
		5	23		5	31 $\frac{1}{2}$	0,90	12	6	21,1	} Ditto		
		7	12		7	20 $\frac{1}{2}$	0,90	12 $\frac{1}{2}$	6	21,1			
		11	5		11	13 $\frac{1}{2}$	0,90	12	6	20,7	} Ditto		
		14	37		14	45 $\frac{1}{2}$	0,90	12	6	20,7			
		17	50		17	58 $\frac{1}{4}$	0,90	11	6	19,8	} Ditto		
		19	50		19	58 $\frac{1}{2}$	0,10	22	1	0,7			
		21	30		21	38 $\frac{1}{2}$	0,10	24	0	59,1	} ☉'s diameter on the scale		
		23	27		23	35	4,60	0 $\frac{1}{2}$	31	29,0			
		25	42		25	50	4,60	1 $\frac{1}{2}$	31	29,8	} Ditto off		
		27	12		27	20	4,60	0 $\frac{1}{2}$	31	29,0			
		28	42		28	50	4,60	2	31	30,2	} ☉'s inclined diameters		
		30	56		31	4	0,90	8					
		35	39		35	47	0,90	5			} Dist. of ♀'s farthest limb from the Sun's nearest		
		44	25		44	32 $\frac{1}{2}$	0,85	19					
		46	14		46	21 $\frac{1}{2}$	0,85	17			} Ditto		
		50	16		50	23 $\frac{1}{2}$	0,85	8					
		57	20		57	27 $\frac{1}{2}$	0,80	20			} Ditto		
		5 32	55	5	32	2	0,70	3	4	46,4			
		34	52		34	59	0,65	23 $\frac{1}{2}$	4	42,6	} Ditto		
		41 $\frac{1}{2}$			41	51 $\frac{1}{2}$	0,15	2	0	50,8			
		42 $\frac{1}{2}$			42	36 $\frac{1}{2}$	0,10	17	0	58,2	} ☉'s diameter on the scale		
											} Ditto off		

1765	Times per clock	Apparent times	Parts of the micrometer	Micro-meter-reduced	Observations on the Transit of Venus continued.
June	h ' "	h ' "	In-ches Ver-nier	' "	
h 5	43 37	5 43 43	0,15 2	0 59,8	♀'s diameter on the scale
	45 $\frac{1}{2}$	45 51	0,10 21	1 1,5	Ditto off
			0,17 2	0 59,8	Ditto on
	53—	53 6 $\frac{1}{2}$	4,60 13 $\frac{1}{2}$	31 34,8	☉'s horizontal diameter
	55—	55 6 $\frac{1}{2}$	4,60 14	31 35,2	
	59 $\frac{1}{2}$	59 21 $\frac{1}{2}$	0,15 4	1 1,5	♀'s diameter on the scale
6	0 41	6 0 47 $\frac{1}{2}$	0,10 18	0 59,1	Ditto off
	1 49	1 55 $\frac{1}{2}$	0,10 21	1 1,5	Ditto ditto
	3 30	3 30 $\frac{1}{2}$	0,50 18	3 56,0	Dist. of ♀'s farthest limb from the ☉'s nearest
	4 40	4 40 $\frac{1}{2}$	0,50 14 $\frac{1}{2}$	3 35,7	Ditto
	6 55	7 1 $\frac{1}{2}$	0,50 11	3 30,3	Ditto
	8 15	8 21 $\frac{1}{2}$	0,50 9	3 29,2	Ditto
	15 0	15 12 $\frac{1}{2}$	0,45 15	3 13,6	Ditto
	17 6	17 12 $\frac{1}{2}$	0,45 8	3 7,9	Ditto
	19 6	19 12 $\frac{1}{2}$	0,45 1	3 2,1	Ditto
	21 5	21 11 $\frac{1}{2}$	0,40 21	2 58,0	Ditto
	25 27	25 33	0,40 6	2 45,7	Ditto
	27 59	27 1	0,40 2	2 42,4	Ditto
	28 19	28 25	0,40 0:1	2 40,8	Ditto } hazy
7	0 40	7 0 45 $\frac{1}{2}$	The thread of light broke at the internal contact		
	0 43	7 0 48 $\frac{1}{2}$	Ditto		
	18 50	7 19 1 $\frac{1}{2}$	The external contact		
	19 15	7 19 20 $\frac{1}{2}$	Ditto		

J. D.

W. W.

W. W.

J. D.

W. W.

J. D.

R E M A R K S.

1. All the measurements of Venus's diameter; and also all those of the Sun, which are not said to be horizontal, were taken with the micrometer, in the same direction that the last preceding distance of the limbs of Venus and the Sun was measured with.

2. We were obliged to alter the rack-work of the micrometer before we began to measure any distances of the limbs, &c. in order to make it take in the diameter of Venus, off the scale.

3. The heavens at the beginning, and for a considerable time both before and after, were frequently obscured by clouds: but in the intervals, the air was very clear, and the Sun's limbs extremely well defined.

4. Soon after Venus was half immersed, a bright crescent, or rim of light, encompassed all that part of her circumference which was off the Sun; thereby rendering her whole periphery visible. This continued very bright until within a few minutes of the internal contact, and then vanished away gradually.

5. We took for the instant of the first internal contact, the time when the least visible thread of light appeared behind the subsequent limb of Venus: but before that time, Venus's limb seemed within that of the Sun, and his limb appeared behind hers in two very obtuse points, seeming as if they would run together in a broad stream, like two drops of oil; but which nevertheless did not happen, but joined in a very fine thread, at some distance from the exterior limb of Venus. This appearance was much more considerable at the egress than at the ingress; owing, as we apprehend, to the bad state of the air at that time. We took for the instant of internal contact, at the egress, the time when the thread of light disappeared before the preceding limb of the planet, from which time W. W. took notice that he had told about 21" when the limbs of the Sun and Venus were apparently in contact: a circumstance which he did not venture to attend to at the ingress.

6. We saw nothing like the appearance of an atmosphere round Venus (unless the above-mentioned phenomena may be thought to proceed from thence) either at the beginning, end, or during the time of the transit: nor could we see any thing of a satellite; though we looked for it several times.

7. It may not be improper to add, that the haziness, complained of at the egress, was not owing to any accidental bad quality of the air at that time; it is continually so here to 10° or 12° above the horizon, and often even to 76° or 18°, in what may be called the clearest state of the heavens.

Observations

Observations for determining the Magnetic Variations at Prince of Wales's Fort on the North-west Coast of Hudson's Bay, by W. W.

The variation compass, which I received from Mr. Robertson, by order of the Royal Society, was, when I received it, a very good one, as appeared to me from several trials which I made of it in London, before it was put on board the ship; but when we arrived in Hudson's Bay, and were ready to make use of it, we had the mortification to find that the needle thereof had, by some cause or other, entirely lost its magnetic virtue. As the cold was, by the time that we made this discovery, much more intense than it probably was at the time that Mr. Ellis complains of a similar circumstance happening to him in those parts, I was naturally led to try whether I could not benefit by his experience, and accordingly removed the compass into the room where we lived; which was kept very warm by a large fire, and by the house stove; and there it remained ever after, but without the least effect.

In order to remedy this misfortune as much as lay in my power, I applied to Captain Richards, as soon as he arrived in the river this year; and desired he would lend me his azimuth compass on shore, with which requests he very kindly complied the next day; but the cloudy weather prevented me from making any observations before the 22d of August.

The compass is of the common form, and I judged that it would be best to make the observations about noon, when the Sun's azimuths change the fastest, and to note the times by the clock; which I did in the following manner:

1769	Times by the clock	Magnetic azimuth	Variation west	
August	^h ^m ^s	^o ['] ["]	^o ['] ["]	
D	21 23 40 29	1 23 W	10 6	These observations were made by Captain Richards; the compass having been removed, and the card re-adjusted after the first
	44 48	2 0 W	9 17	
	49 35	3 20 W	9 2	
	52 53	4 30 W	9 6	
a	22 0 6 40, 1			The Sun transited the meridian
	23 18	15 15 W	9 43	By myself; the compass having been moved, and the card re-adjusted
	25 22	16 17 W	10 3	
	27 8	16 39 W	9 50	
	23 28 59	2 38 E	9 49	The compass placed as it was yesterday before noon
	31 15	1 50 E	9 52	
	34 5	0 47 E	9 59	
	42 20	2 0 W	10 3	The compass removed, &c.
	44 10	2 38 W	10 4	
	45 40	3 12 W	10 7	
	49 45	4 21 W	9 57	Ditto
	52 44	5 20 W	9 51	
	54 4	5 40 W	9 50	
23	0 6 38, 6			The Sun transited the meridian
	12 43	11 25 W	9 24	The compass removed, &c.
	25 13	15 28 W	9 18	
	26 26	16 7 W	9 33	
	28 8	16 30 W	9 22	The compass again removed, and the card re-adjusted
	34 38	18 40 W	9 24	
	36 16	19 22 W	9 33	
				Ditto
The mean is			9 41	

Such are the best observations of this kind, which I am able to lay before this honourable and learned Society. It gives me much concern to find that they differ so widely from one another; more especially as I am certain that I made them with all the care and circumspection that I was capable of, and with an instrument which seemed to me good of its kind. But I flatter myself it will be considered, that, in making observations with this instrument, there are two unavoidable sources of error, viz. in adjusting the card to the line on the side of the compass-box, and in making the shadow of the thread to fall on the line of the index: I may likewise add a third error, which may be committed in reading of the vernier, as it only subdivides to every 5'; and if all these should happen to fall the same way, their sum, I presume, may be considerable (when an instrument of so small a radius is used), in the hands of the most skilful observer.

The Latitude of Prince of Wales's Fort on the North-west coast of Hudson's Bay, deduced from Observations of circumpolar Stars.

1769 By ζ Ursæ Majoris				1769 By Capella			
Date of the Observation.		Latitude deduced		Date of the Observation		Latitude deduced	
Above the pole	Below the pole	90 Arch	96 Arch	Above the pole	Below the pole	90 Arch	96 Arch
		o' " "	o' " "			o' " "	o' " "
Jan. 20	Jan. 18	58 47 29 $\frac{1}{2}$	58 47 53	Jan. 29	Jan. 28	58 47 23	58 47 36 $\frac{1}{2}$
	20	58 47 27	58 47 51	30		58 47 23 $\frac{1}{2}$	58 47 31 $\frac{1}{2}$
29	18	58 47 33 $\frac{1}{2}$	58 47 52	31		58 47 24 $\frac{1}{2}$	58 47 30 $\frac{1}{2}$
	20	58 47 29	58 47 50	Febr. 5		58 47 28 $\frac{1}{2}$	58 47 59 $\frac{1}{2}$
The means of these are		58 47 30 $\frac{1}{2}$	58 47 51 $\frac{1}{2}$	8		58 47 34 $\frac{1}{2}$	58 47 48 $\frac{1}{2}$
1769 By γ Ursæ Majoris				9		58 47 31 $\frac{1}{2}$	58 47 37
Jan. 20	Jan. 18	58 47 51	58 48 6	Jan. 29	Jan. 29	58 47 30 $\frac{1}{2}$	58 47 44 $\frac{1}{2}$
	21	58 47 42	58 48 7	30		58 47 30 $\frac{1}{2}$	58 47 38 $\frac{1}{2}$
	29	58 47 46 $\frac{3}{4}$	58 48 2 $\frac{1}{2}$	31		58 47 31 $\frac{1}{2}$	58 47 38
	30	58 47 53	58 48 7 $\frac{1}{2}$	Febr. 5		58 47 34 $\frac{1}{2}$	58 47 46 $\frac{1}{2}$
29	18	58 47 14	58 47 30 $\frac{1}{2}$	8		58 47 41 $\frac{1}{2}$	58 47 51 $\frac{1}{2}$
	21	58 47 14 $\frac{1}{2}$	58 47 30 $\frac{1}{2}$	9		58 47 40 $\frac{1}{2}$	58 47 44 $\frac{1}{2}$
	29	58 47 9 $\frac{1}{2}$	58 47 36 $\frac{1}{2}$	Jan. 29	Febr. 10	58 47 22 $\frac{1}{2}$	58 47 35 $\frac{1}{2}$
	30	58 47 15 $\frac{1}{2}$	58 47 32 $\frac{1}{2}$	30		58 47 23	58 47 30 $\frac{1}{2}$
Means of these are		58 47 32	58 47 48	31		58 47 24	58 47 29 $\frac{1}{2}$
				Febr. 5		58 47 26 $\frac{1}{2}$	58 47 38 $\frac{1}{2}$
				8		58 47 33 $\frac{1}{2}$	58 47 43 $\frac{1}{2}$
				9		58 47 32 $\frac{1}{2}$	58 47 36
				14		58 47 32 $\frac{1}{2}$	58 47 32 $\frac{1}{2}$
				Jan. 29	Febr. 13	58 47 21	58 47 34 $\frac{1}{2}$
				30		58 47 21 $\frac{1}{2}$	58 47 30 $\frac{1}{2}$

1769 By Capella continued

Date of the Observation		Latitude deduced					
Above the pole		Below the pole					
		90 Arch			96 Arch		
		°	'	"	°	'	"
Jan.	31	58	47	22 $\frac{1}{2}$	58	47	29 $\frac{3}{4}$
Febr.	5	58	47	25 $\frac{1}{2}$	58	47	38 $\frac{1}{2}$
	8	58	47	32 $\frac{1}{2}$	58	47	43 $\frac{1}{2}$
	9	58	47	30 $\frac{1}{2}$	58	47	36
	14	58	47	31 $\frac{1}{2}$	58	47	32 $\frac{1}{2}$
	24	58	47	31 $\frac{1}{2}$	58	47	36 $\frac{1}{2}$
Means of these are		58	47	29	58	47	37 $\frac{1}{2}$

By α Persei

Jan.	29	Jan.	29	58	47	23 $\frac{3}{4}$	58	47	28 $\frac{1}{2}$
	31			58	47	28 $\frac{1}{2}$	58	47	33 $\frac{1}{2}$
Febr.	14			58	47	33	58	47	38
Means of these are				58	47	28 $\frac{1}{2}$	58	47	33 $\frac{1}{2}$

* * These four stars passed the meridian to the southward of the zenith, when above the pole; which circumstance rendered them vastly convenient for determining the lati-

tude of the place, as the error of the line of collimation of the quadrant is thereby entirely excluded, provided it did not alter in the interval between the observations.

1768 By the Pole Star

Date of the Observation		Latitude deduced						
Above the pole	Below the pole	90 Arch			96 Arch			
		°	'	"	°	'	"	
Decem.	1769							
	January 1	58	47	25½	58	47	37	
	Febr. 8	58	47	6	58	47	24½	
		24	58	47	6½	58	47	24
	3	January 1	58	47	32½	58	47	33
	Febr. 8	58	47	7½	58	47	24½	
		24	58	47	8½	58	47	24
	15	January 1	58	47	37	58	47	38½
	Febr. 8	58	47	18½	58	47	26	
		24	58	47	18	58	47	25½
	19	January 1	58	47	25½	58	47	37½
	Febr. 8	58	47	7½	58	47	25	
1769								
	Jan. 24	58	47	8½	58	47	24½	
	18	January 1	58	47	25½	58	47	35½
	Febr. 8	58	47	6	58	47	23	
		24	58	47	7½	58	47	22½
Means of these are		58	47	16	58	47	28½	

The Latitude of Prince of Wales's Fort deduced from Observations of the Sun, and of such stars as passed south of the zenith.

By the Sun		Latitude deduced			
1768	Declination	90 Arch		96 Arch	
	° ' "	°	'	°	'
Sept.	N.				
	20 0 42 9	58	48 19	58	47 57
	21 0 18 44 $\frac{1}{2}$	58	48 26 $\frac{1}{2}$	58	48 14 $\frac{1}{2}$
	S.				
	22 0 4 40	58	48 15 $\frac{1}{2}$	58	48 2
	30 3 11 59 $\frac{1}{2}$	58	48 15 $\frac{1}{2}$	58	48 3
Oct.	1 3 35 19 $\frac{1}{2}$	58	48 21 $\frac{1}{2}$	58	48 33
	14 8 32 49	58	48 18 $\frac{1}{2}$	58	48 12
	17 9 39 4	58	48 32 $\frac{1}{2}$	58	48 7 $\frac{1}{2}$
1769	N.				
June	20 23 28 5 $\frac{1}{2}$	58	47 38 $\frac{1}{2}$	58	47 26
	22 23 27 40 $\frac{1}{2}$	58	48 10 $\frac{1}{2}$	58	47 46
August	2 17 36 18 $\frac{1}{2}$	58	48 12 $\frac{1}{2}$	58	48 6 $\frac{1}{2}$
	5 16 48 8 $\frac{1}{2}$	58	48 16	58	48 7
	22 11 33 11	58	48 14	58	47 27 $\frac{1}{2}$
The means of these are		58	48 15	58	48 0

By Capella		Latitude deduced			
1768	Declination	90 Arch		96 Arch	
	° ' "	°	'	°	'
Oct.	13 15 44 10,3	58	47 34 $\frac{1}{2}$	58	47 33 $\frac{1}{2}$
	14	58	48 32	58	47 40 $\frac{1}{2}$
	17	58	47 48	58	47 39 $\frac{1}{2}$
The means of these are		58	47 48 $\frac{1}{2}$	58	47 39 $\frac{1}{2}$

By α Persei

Sept.	29 49	1 9	58	47 47	58	47 53 $\frac{1}{2}$
Oct.	14 49	1 10	58	47 47	58	47 39 $\frac{1}{2}$
	17		58	47 44	58	47 21 $\frac{1}{2}$
The means of these are			58	47 46	58	47 38 $\frac{1}{2}$

1768.

By α Lyrae			Latitude deduced		
1768	Declination		90 Arch	96 Arch	
	°	' "	°	' "	°
Oct. 16	38	34 49	58 47 21 $\frac{1}{2}$	58 47 31 $\frac{1}{2}$	
17			58 47 24	58 47 35 $\frac{1}{2}$	
The means of these are			58 47 22 $\frac{1}{2}$	58 47 33 $\frac{1}{2}$	

By α Aquilæ

Sept. 29	8 16 22 $\frac{1}{2}$	58 47 51 $\frac{1}{2}$	58 47 18 $\frac{1}{2}$
Oct. 13		58 47 52	58 47 23
14		58 48 6	58 47 44
16		58 48 11	58 47 48 $\frac{1}{2}$
The means of these are		58 48 0	58 47 33 $\frac{1}{2}$

By α Cygni

Oct. 24	44 27 48	58 47 21	58 47 0
13		58 47 26 $\frac{1}{2}$	58 47 17
16		58 47 32	58 47 32
17		58 47 38	58 47 37
The means of these are		58 47 29 $\frac{1}{2}$	58 47 21 $\frac{1}{2}$

By α Persei

Nov. 16	49 1 11	58 47 50	58 47 20 $\frac{1}{2}$
18		58 47 29	58 47 12 $\frac{1}{2}$
19		58 48 11	58 48 2
28		58 48 0	58 47 34
Decem. 10		58 48 14	58 48 2
The means of these are		58 47 57	58 47 38

By Capella			Latitude deduced					
1768		Declination	90 Arch			96 Arch		
		° ' "	°	'	"	°	'	"
Nov.	16	45 44 10 $\frac{1}{2}$	58	47	54,2	58	47	29,0
	18		58	47	43,2	58	47	12,2
	28		58	47	26,7	58	47	18,5
	29		58	47	38,9	58	47	32,1
The means of these are			58	47	40,7	58	47	23,7

1769 By β Draconis

June 24	52 28 50	58 47 59	58 47 36
July 20		58 48 14	58 47 55
21		58 48 13	58 48 5
22		58 47 58	58 47 45
The means of these are		58 48 6	58 47 50 $\frac{1}{2}$

By γ Draconis

June 24	51 31 29 $\frac{1}{2}$	58 47 37	58 47 29
July 2		58 47 38 $\frac{1}{2}$	58 47 40
7		58 47 43	58 47 46 $\frac{1}{2}$
18		58 47 50 $\frac{1}{2}$	58 47 43
20		58 47 44	58 47 40
21		58 47 43	58 47 44
22		58 47 48 $\frac{1}{2}$	58 47 46
28		58 47 50	58 47 48
30		58 47 40 $\frac{1}{2}$	58 47 39
August 4		58 47 48 $\frac{1}{2}$	58 47 41
The means of these are		58 47 44 $\frac{1}{2}$	58 47 41 $\frac{1}{2}$

The Latitude of Prince of Wales's Fort deduced from Observations of Stars on the Northern Meridian.

By α Ursa Majoris		Latitude deduced		
1768	Polar diff.	90 Arch	96 Arch	
	°	' "	°	' "
Decem. 2	27 0 24	58 47 14	58 47 21	
3		58 47 3	58 47 12	
6		58 46 57	58 47 14	
15		58 47 8	58 47 20	
The means of these are		58 47 5 $\frac{1}{2}$	58 47 16 $\frac{1}{2}$	

By γ Ursa Majoris		Latitude deduced		
1768	Polar diff.	90 Arch	96 Arch	
	°	' "	°	' "
Decem. 15	35 1 15	58 46 50	58 47 10	
		58 46 58	58 47 8	
		58 46 59	58 47 19	
The means of these are		58 46 55 $\frac{2}{3}$	58 47 12 $\frac{1}{3}$	

By γ Urfæ Majoris		Latitude deduced	
1769	Polar diff.	90 Arch	96 Arch
	o ' "	o ' "	o ' "
January 1	59 31 35	58 47 10	58 47 8
2		58 47 2	58 47 14
The means of these are		58 47 6	58 47 11
The means of all the comparisons of ξ Urfæ Majoris		o ' "	o ' "
Ditto of γ Urfæ Majoris (considered as circumpolar)		58 47 30 $\frac{1}{2}$	58 47 51 $\frac{1}{2}$
Ditto of Capella ditto		58 47 32	58 47 48
Ditto of α Persei ditto		58 47 29	58 47 37 $\frac{1}{2}$
Ditto of the Pole star		58 47 28 $\frac{1}{2}$	58 47 33 $\frac{1}{2}$
		58 47 16	58 47 28
The means of all the circumpolar stars are		58 47 27	58 47 39 $\frac{1}{2}$
The means of α Urfæ Majoris		58 47 5 $\frac{1}{2}$	58 47 16 $\frac{1}{2}$
Ditto of γ		58 46 53 $\frac{1}{2}$	58 47 12 $\frac{1}{2}$
Ditto of η		58 47 6	58 47 11
The means of all the stars taken on the northern meridian are		58 47 24	58 47 13 $\frac{1}{2}$
And the means of the above two are		58 47 14 $\frac{1}{2}$	58 47 26 $\frac{1}{2}$
The means of all the solar observations are		58 48 15	58 48 0
Ditto of Capella taken on the southern meridian alone		58 47 48 $\frac{1}{2}$	58 47 39 $\frac{1}{2}$
Ditto of α Persei		58 47 46	58 47 38 $\frac{1}{2}$
Ditto of α Lyrae		58 47 22 $\frac{1}{2}$	58 47 33 $\frac{1}{2}$
Ditto of α Aquilæ		58 48 0	58 47 33 $\frac{1}{2}$
Ditto of α Cygni		58 47 29 $\frac{1}{2}$	58 47 21 $\frac{1}{2}$
Ditto of Capella after the line of Collimation altered		58 47 40 $\frac{1}{2}$	58 47 23
Ditto of α Persei ditto		58 47 57	58 47 38
Ditto of β Draconis, the line of collimation having again altered		58 48 6	58 47 50 $\frac{1}{2}$
Ditto of γ ———		58 47 44 $\frac{1}{2}$	58 47 41 $\frac{1}{2}$
The means of all the observations taken southward of the zenith are		58 47 49	58 47 38
The means of the circumpolar and northern stars		58 47 14 $\frac{1}{2}$	58 47 26 $\frac{1}{2}$
And, by taking the mean of both, the latitude is		North 58 47 32	58 47 32 $\frac{1}{2}$

The error of the line of collimation of the quadrant was 25".6 for the 90 arch, and 19".7 for the 96 arch, to be subtracted from all zenith distances, from the beginning of September, 1768, to the latter end of October; from about which time, till towards the latter end of December, it appears to have been 29".4 for the 90° arch, and 36".7 for the 96 arch, to be added to all zenith distances taken in that interval. About the latter end of December it altered again, but I had no opportunity of determining its quantity, and seemed to be pretty constant all the month of January, 1769; but, about the beginning or middle of February, it began again to alter, and continued uncertain until the middle or latter end of June, when it became constant again, and seemed to me to be, by the observations of β and γ Draconis, 21".6 and 15".5 to be subtracted from the 90 and 96 arches, respectively.

A TABLE of the EQUATIONS to Equal Altitudes. Lat. $58^{\circ} 47\frac{1}{2}'$.

Half the Interval between the Observations.

[illegible]

The instruments used in making the preceding observations were :

1. A clock, made by Mr. Ellicot, with an apparatus for correcting the effects of heat and cold; the same which Messieurs Maſon and Dixon had to the Cape of Good Hope in the year 1761.
2. An astronomical quadrant, made by Mr. Bird, of one foot radius.
3. Two reflecting teleſcopes, of two feet focus, made by Mr. Short; and a divided object-glaſs micrometer, made by the ſame gentleman, of 501,45 inches focal length.

We used the micrometer with a magnifying power of 60; the contacts of Venus with the Sun's limb were observed with a magnifying power of 120, and all the other observations with one of 90.

Both the thermometers, used in the preceding observations, were according to Fahrenheit's scale; and the characters + and —, which are annexed to their altitudes, denote that they stood so many degrees above or below the cypher respectively: where neither of those characters appears, the number is to be understood above the cypher.

LXVI. *Extract from the Journals of the Royal Society, June 23, 1768, respecting a Letter addressed to the Society by a Member of the House of Jesuits at Pekin in China; by Charles Morton, M. D. Sec. R. S. and Fellow of the Imperial Acadd. Natur. Curios. & Petropol. and of the Royal Academy of Gottingen.*

THIS letter, the original of which is in the French language, consists of 28 pages in close folio; to which are subjoined 44 pages of notes; and 27 pages of drawings, to which the letter and notes refer for illustration.

It relates to some disquisitions of Mr. Turberville Needham, F. R. S. concerning a supposed connection between the hieroglyphical writing of antient Egypt, and the characteristic writing which is in use at this day, amongst the Chinese.

Divers of the Society remember Mr. Needham's tract upon this subject, which was printed at Rome in the Latin tongue, 1761, addressed to this and the Antiquarian Societies.

This conjecture of Mr. Needham's, pregnant with so many curious consequences, engaged the attention of the Literati of Europe: the generality wishing

success to it; and divers, either from a particular information, or for other reasons, opposing it. Mr. Desguignes of Paris, F. R. S. Mr. Bartoli of Turin, antiquary to the King of Sardinia; the late Abbé Winkelman, antiquary to the Pope, and Mr. Montagu, F. R. S. were the principal of those who thought themselves concerned to oppose Mr. Needham; and what they have been pleased to communicate, either in print or manuscript, has been already laid before the Society; and the last gentleman, viz. Mr. Montagu, has also sent to England a cast of the bust of Turin, inscribed with certain characters, which gave occasion to Mr. Needham's conjectures: which cast, by the bounty of his Majesty our Patron, is now in the British Museum.

The subject in question seemed sufficiently interesting to seek an answer from the only competent judges, the literati of China; and your Secretary, first by the encouragement of Thomas Hollis, Esq; F. R. S. and subsequently by the assistance of Thomas Wilcocks, Esq; F. S. A. and the particular favour of the Directors of the East-India Company, has at length obtained it.

In order to this, a letter was written, in conjunction with Mr. Alban Butler, late of Pall-Mall, (who had some interest among the Jesuits at Pekin) stating the matter in question, and desiring the favour of an answer; which answer is the letter that has been read to the Society.

The particulars which were stated to the Jesuits at Pekin, and have been recited to the Society, were as follows, viz. 1. Whether certain characters, to the number of 29, copied from the bust at Turin, together

ther with divers other characters, to the number of 200, copied from undoubted monuments of Egypt, are really and indeed Chinese characters; and if they be, of what dialect, and of what age are they?

2. What sense doth each of these characters express; and what is the particular interpretation?

3. Doth the history of China, or popular tradition, or any analogy with the modern or antient method of writing of any other nation, afford ground for supposing that these characters have been received from foreigners; or were they invented by the Chinese themselves?

4. Are there any monuments or customs amongst the Chinese, which resemble those of the antient Egyptians; or which should induce us to think, that there has ever been any communication between the two nations?

The answer received from China takes notice only of the small number of characters which were copied from the bust of Turin; occasioned probably by some accident or failure in the packets, of which there were three copies sent, and one of them containing the Turin characters only; the answer is dated from Pekin, October 20, 1764, addressed to the Members of this Society, but with no subscription, or signature, excepting four stars, and this addition of *the company of Jesus*.

The author's method, or order, is as follows:

1. An introductory preface. 2. A state of the enquiry, as collected partly from the letter, and partly from Mr. Needham's printed book. 3. What the author calls an historical picture of the Chinese tongue and its characters. 4. An applica-

tion of this historical delineation, in the way of principles, to decide concerning the 29 characters of the bust of Turin. 5. A more general application of the said principles, in order to elucidate the hieroglyphical writing, and consequently the antiquities of Egypt, by a proposed collation with the antient symbolical writing of China, exemplified by divers instances: and lastly the notes, containing circumstantial details of some particulars, as well historical as critical, which might otherwise have broken the thread of the letter.

I shall not pretend to give an adequate idea of this curious paper, within the compass of an extract suited to this place.

The particular branch discussed in this letter, as well as the general learning of China, are subjects in a manner new to Europe; and the various books of the Chinese are called by the author a *Potofsi*, which might enrich Europe; especially with regard to laws, government, the useful arts, natural history, and the like. Some strictures from the letter are to this effect: and,

1. In the preface mention is made of the insufficient attempts of the Greeks and Romans to explain the hieroglyphical writing of Egypt; and of the later attempts of father Kircher, and Mr. de Mairan, who anticipated Mr. Needham in the idea of explaining them by the characteristic writing of China; which idea they quitted almost as soon as they had formed it.

2. The state of the question is expressed as follows: " Mr. Needham has observed, that the symbols or hieroglyphical characters of the Isis of Tu-

" rin,

“ rin, appear like several Chinese characters, such as
 “ they are found in the great dictionary Tching,
 “ tsee, tong: upon which he conjectures, first, that
 “ the Chinese characters are the same in many re-
 “ spects, as the hieroglyphics of Egypt; and second-
 “ ly, That one may be able to discover the sense of
 “ hieroglyphics by the comparative and appropriated
 “ signification of the Chinese characters.”

The author, thirdly, having mentioned the difficulty of rendering himself intelligible to the literati of Europe, for want of a certain acquaintance with proofs of fact and history, criticism and grammar, proceeds to the historical detail, which is the subject chiefly enlarged upon by him. He notes the enthusiasm of Vossius for the antiquity of the Chinese, and the rage of Renaudot against it; and then declares his own opinion, that they have subsisted as a nation, from the time of the great emigration which followed the confusion of tongues. He dates the antiquity of Egypt from the same epoch, and gives reasons, particularly in the notes, for the probability of their different routs. He enquires into the use of writing; and declares his opinion, that it was already established in the antediluvian world; and might be derived in common to the two nations in question. He makes light of any supposed variation of it, at the confusion of tongues: and wishes that Mr. Needham had expressed his own opinion concerning the commencement of it. He affirms, that there is not the least mark or trace now remaining of any subsequent communication between the Chinese and Egyptians. But whether our author's opinion of the origin of writing, or the contrary one, of each nation having invented
 its

its own, be adopted: he candidly owns, that any connection between the two modes of writing, is hardly discernible at this day. He affirms, that the Chinese language is one of the most antient; and perhaps the only one which has been spoken without interruption; and is yet a living language; the small number and the shortness of its words having so guarded it from changes, that they could scarcely extend farther than the pronunciation.

They distinguish in the Chinese language, 1st, the Kou-ouen, the language of the *King*, and other books written in this taste. The harangues of the *Chou-king*, and the songs of the *Chi-king*, prove that it was spoken formerly. It is prodigiously laconick. 2dly, The *Ouen-tchang*, the language of *relcvees*, elevated compositions, and books. This language, excepting some proverbs, axioms, and forms of compliment, is no longer used in speaking. 3dly, The *Kouan-hoa*, the language of men in office. This is the only language spoken at court, and in good company, and used in books; and this alone runs through the empire. 4thly, The *Hiang-tan*, patois or provincial jargon. Each province and town, and almost every village has its own. In spite of these varieties, the Chinese tongue counts but about 330 words. From hence the Europeans conclude, that it is barren, monotone, and hard to understand. But they ought to know, that the four accents called *ping*, uni (*even*) *chung*, élevé, (*raised*), *kiu* diminué (*lessened*), *jou*, reentrant, (*returning*), multiply almost every word into four, by an inflexion of voice which it is as difficult to make an European comprehend, as it is for a Chinese to comprehend the six pronunciations of the French E.

These

These accents do yet more; they give a certain harmony, and pointed cadence, to the most ordinary phrases: with regard to clearness, let fact decide. The Chinese speak as fast as we do, say more things in fewer words, and understand one another.

The Chinese have no distinct knowledge of the invention of writing; as our author proves by quotations from their most antient books; which are involved in fable, and contradict each other. One of them indeed mentions this curious particular; that Fou-hi, by introducing the eight Koua, or elementary characters, put an end to the use of knots upon cords, for the purposes of government; which seems to be analogous to what has been observed in America.

The author defines the Chinese characters, according to his conception of them in their origin, to be images and symbols which speak to the mind by the eyes. Images, for sensible things; symbols, for mental. Images and symbols which are tied to no sound, and may be read in every tongue. The book *Tsee-bio-leang-tsin* divides the characters into six sorts, *Lieou-y*. The first, called *Siang hing*, shape, image, is a true picture of sensible things. Thus one sees in the antient characters, trees, birds, vases, &c. rudely traced out.

The 2d, called *Tchi-che*, indication of the thing, is made by an addition to the shape, or to the symbol, which puts the thing that one would express before the eyes. For example, the character of *small* placed over that of *great*, to signify *pyramidal, terminated in a point*. The 3d, called, *Hoei-y*, *junction of idea, association*, consists in joining two characters, to express a thing which neither the one nor the other signify separately.

separately. For example; the shape of *mouth* placed aside that of *dog*, to signify the verb *bark*. The 4th, *Kiai-in*, *explication*, or *expression*, of *the sound*, owes its origin to the difficulty of tracing in a manner sufficiently distinct, all the sorts of fishes, animals, vases, trees, &c. To supply this, they contrived to place the simple character of one sound on the side of the figure. For example; the character of the sound *ya* on the side of the figure of a bird, to signify a *duck*: the character of *ngo*, to signify a *goose*.

The 5th, called *Kia-sie*, *idea borrowed*, *metaphor*, hath opened an immense field to the invention of characters; or rather, to the manner of making use of them. In effect, by virtue of the *Kia-sie*, one character is sometimes taken for another; chosen to express a proper name; turned aside to a sense allegorical, metaphorical, ironical; and pushed even to an antiphrasis, in giving it a sense opposite to that wherein it is employed elsewhere. It must be owned, that this 5th class gives the Chinese tongue a force, and a vivacity of colouring, that no other tongue can attain.

But it is also one of the principal causes of its obscurities. The figurative sense of a character has not always a due analogy with the proper sense.

The 6th, called *Tchouen-tchou*, *developement*, *explication*, consists only in extending the primitive sense of a character, or in making detailed applications of it. Thus the same character is sometimes verb or adverb, sometimes adjective or substantive. Thus again, the character *ngo*, which signifies *evil*, serves to express *hatred*, *to hate*, *mishapen*, &c.


These six, *Lieou-y*, such as here described, are as it were the sources from whence flow all the characters in

in a manner equally simple, clear, and natural; and the whole number of characters is thus increased from 300, to 80,000. And, the author subjoins, “ One must read the fine passages of the *King*, to comprehend what force, grace, energy, amenity, grandeur, and simplicity, the Chinese characters have, where they are well assorted, and well connected.

“ I would willingly define the Chinese characters to be the picturesque algebra of the arts and sciences. In truth, a phrase of good style is as disembarrassed of every thing that is intermediary, as the closest algebraic demonstration.”

Unless one were to give the lye to the Chinese, and to the small number of the characters of antient times which they have preserved; it is not possible to deny, that they did, in the most remote antiquity, make use of shapes, or likenesses of sensible things, and of symbols to form their characters, nearly in the taste of the hieroglyphics of Egypt: and one need but cast the eye on some of the characters which are copied in the pages 5, 6, and 7, (TAB. XXIV; XXV, XXVI.) of the plates following, to be convinced thereof. But had not the Chinese, even from that time, the art of contracting these figures, and reducing them to some strokes or lines, by analysis and abbreviation? To judge thereof by some of the antient characters, it appears, that the Chinese did reduce several to certain strokes ill enough assembled; probably for the conveniency of writing. And whensoever the time was wherein the abbreviations began, they were necessary; 1st, because without them, writing would have been too difficult: 2dly, because one must have had volumes to convey a very

small matter. In effect, without being well versed in drawing, how could one trace in an agreeable manner so many figures and symbols? The difficulty augments when one reflects, that a good many characters were composed of divers symbols and images, the reduction of which ought to be very well touched, not to be disagreeable; especially near to other characters that were less compound. It is natural to think, that they would not make use of images and symbols intire, and traced in their just proportion, but for great monuments, where room was not wanting. And yet it should not be denied, that they had recourse to the analysed characters, for certain places less advantageous.

The fact established by what remains of the Chinese monuments is, that the shapes and the symbols have passed from a contour sufficiently regular, to some lines oddly assembled; and that the lines themselves have been yet decomposed, and melted, into these six lines,  out of which, at this day, are composed all the characters in use. The simplest are made of one or two of these lines; and they count as far as 20 or 30, or more lines, in the more compound characters. To avoid the confusion and obscurity which this great abbreviation would have caused, they have fixed the number of the lines of the characters which represent the 200 elementary images and symbols spoken of. These abbreviations thus fixed are called *Pou*, *Classes* or *Tribunals*, as Mr. Fourmont translates. For example; the *Pou* of man, of woman, of trees, of diseases, of great, of small, of vase, &c. In brief, for greater clearness, and to range the characters in the dictionaries, there is in each character

ter a distinctive or differencing *Pou*, which predominates, and under which the character is placed. This differencing *Pou* is the part of the character which hath most influence in its signification; saving the exceptions, and oddities, from which the Chinese is no more exempt than other tongues. A bare inspection into the dictionary *Tching-tsee-tong*, will render these details intelligible.

The misfortune, and a very great misfortune, of the Chinese characters is, that these abbreviations have been made by little and little, in different places, and without rule: so that there are characters which have been abridged, or more properly truncated, and disfigured a very great number of ways: and the most part, so much, as to be no longer knowable by the primitive form. To give some idea of this, the author has caused to be copied the variations of four characters (see the plates 7, 8, 9. TAB. XXVI, XXVII, XXVIII.); and one may judge by this sample, how frightfully disfigured must be those characters which are woven out of several other characters. For the different characters which are thus united to make one only, are curved, lowered, lengthened, drawn in, or contracted, to the end that each line may be so placed, as that all together may make the contrast of a simple character, and occupy no more space than it does. A like constraint ought to disfigure many of the elementary characters which are joined together to make one only. But when we add thereto the abbreviations and various readings, it is clear that they can no longer be knowable by their primitive characters. And this, to observe it *en passant*, is one of the reasons which has rendered the edition of the *King* under the *Han* so difficult, and

perhaps is the principal cause of their obscurity. In effect, the primitive images and symbols being altered, how can one find the sense of them? It is no more according to the rule of the *Lieou-y*. The decomposition of the elementary characters whereof it is composed, no longer gives its true analysis. The more one seeks the sense which ought to result from their assemblage, the farther one is from it: because that this assemblage is not the true one. It is as if one should read (in French) *delires* for *delices*. This change of the *c* into *r* subsisting, all the significations that one shall seek to *delires*, will never arrive at the idea presented by *delices*.

If the comparison is lame, it is because that it represents not sufficiently clearly how far a Chinese character separates from its true signification, by the alteration of some one of the lines that compose it. The destruction of the books by fire has rendered the evil without a remedy. When peace was restored to letters, they spared neither care nor inquiry to recover the *King*, and other antient books. But few copies having escaped the flames, and those not in the best preservation, they were deprived of the great advantage to be drawn from collations, to discover the primitive characters. Writing had changed; tradition was almost extinguished. It was necessary to be learned, even to decypher the manuscripts: how should they be able to pursue the discussion so far as the various readings; and unravel, amongst abbreviations almost unknown, the true symbols and likeness of which a character was woven. The editors were not sparing of their labour herein; but each had his system, and his conjectures. Who would venture

ture to say, that the edition which has prevailed has not many mistaken characters? and let it be even the best, learned men, who have laboured since in the analysis of the characters, are not agreed amongst themselves; and they bring each reasons capable of suspending the judgement of critiques. This variety of opinion hath caused much variety in the *orthography*, if one may so call the manner of writing a character with such or such a *Pou*. The manner accordingly has been floating and uncertain, for very many characters, until the great dictionary *Kang-hi-tse-tien*, which has fixed it.

The author winds up this curious detail with the following remark, which he says is essential. All that has been said of the various readings and abbreviations of the characters is independent of the five sorts of writing ordinarily counted by lettered men. The first is called *Kou-ouen* (see plates the 5th, 6th, 7th, and 8th, and part of the 9th, TAB. XXIV—XXVIII.) This is the most ancient form of writing; and there remains now hardly any more traces of it. The second, *Tchoang-tsee*, (also read *Tchouen-tsee*, vid. plate 1. TAB. XIX.) has succeeded the *Kou-ouen*; and has lasted even to the end of the Dynastie of the Tcheou. It was this which was in use from the time of Confucius, and of which the abbreviations and various readings have been most fatal. The third, *Li-tsee*, (see plate 2. TAB. XXI.) began under the reign of *Chi-boang-ti*, the founder of the Dynastie of the Tsin, and the great enemy of letters and of lettered men. The fourth, *Hing-chou*, is destined for impresson, as with us the Roman and Italic. (See plate 3. TAB. XXII.)

The

The fifth sort, *Tsao-tsee*, (see plate 4. TAB. XXIII.) was invented under the *Han*, and would have destroyed every thing, if it had prevailed. It is a sort of writing with the stroke of a pencil, with a very light and well-experienced hand : but it disfigures the characters beyond expression. It has no course, but for the prescriptions of physicians, prefaces of books, inscriptions of fancy, &c.

To return to the various readings, and abbreviations; although it be true that these different sorts of writing have augmented the number of them; nevertheless the three last have done no great harm; because they have been directed by learned men, consecrated by publick authority, and bear more on the general form of the characters, than on their orthography. Thus the literati do not complain, further than their having caused the loss of the antient characters, which it would have been well to consult, to have had the true analysis of several of the characters of this day, which they think ill written, and disfigured.

And thus, at length, having compleated his historical detail, (which I have here represented very imperfectly) our author decides concerning Mr. Needham, viz: that the characters of the bust of Turin, (though four or five of them, viz. N^o 2, 3, 8, 9, 31, have a sensible resemblance to the like number of characters in the Chinese dictionary), are not genuine Chinese characters; having no connected sense, nor a proper resemblance to any of the different forms of writing; indeed the whole inscription has nothing of Chinese in the face of it. As a farther proof, our author took the opinion of divers of the Chinese literati, whose province it is to study the antient writings;

tings; who all declared the same thing; and that they did not understand them, nor had ever seen the like of them.

It is owned, however, that, according to the Chinese interpretation of the five resembling characters, they are simple ideas, or symbols, not characterized by the farther circumstantiating lines; and are, without coherence, in the way of Nomenclator.

But finally, to enable the Society to judge for themselves; our learned correspondent has sent a collection of very antient inscriptions, above one hundred in number, which may be compared with the inscription of Turin; as also, some drawings of vases, and other antiquities. See plates, from 13 to 27 (TAB. XXII to XLVI.) inclusive. The particular matter of enquiry, viz. the characters of the bust of Turin being thus disposed of, our author, who is against renouncing Mr. Needham's general conjecture, without farther examination, as it may notwithstanding conduct to many discoveries, applies himself, fifthly, to a farther and more general investigation, by an actual collation of such Egyptian hieroglyphics as do undoubtedly resemble antient characters, yet remaining amongst the Chinese: in order to which, he has given us drawings of 73 such hieroglyphics, collected chiefly from Kircher (as he had no better materials), and has placed by them the corresponding Chinese characters, (see plates, from 9 to 12, TAB. XXVIII to XXXI inclusive) both antient and modern. He is sufficiently diffuse and curious, in two or three examples, to point out the method and most interesting subjects of enquiry, viz. the leading notions concerning the Deity, and the religion of the primitive times; and he also describes the properties
of

of the symbolical animals, which are supposed to be significant of the rational and moral qualities; but enters a caution against these, as being, most likely, the invention of later times. He argues strenuously for the early and uninterrupted Theism of the Chinese; and concludes with an apology for the condition of a missionary, the duties of whose profession, and separation from divers necessary means of information, render him, in his own opinion, very unfit for literary inquiries.

C. Morton.

The characters called T-bowen-tee.

𐤀	𐤁	𐤂	𐤃	𐤄	𐤅
𐤆	𐤇	𐤈	𐤉	𐤊	𐤋
𐤌	𐤍	𐤎	𐤏	𐤐	𐤑
𐤒	𐤓	𐤔	𐤕	𐤖	𐤗
𐤘	𐤙	𐤚	𐤛	𐤜	𐤝
𐤞	𐤟	𐤠	𐤡	𐤢	𐤣
𐤤	𐤥	𐤦	𐤧	𐤨	𐤩
𐤪	𐤫	𐤬	𐤭	𐤮	𐤯

The character called Li T'ieh.

人 舊 藏 格 什 匹 篆

亓 有 兵 制 百 州 圓

編 福 豷 歐 蒙 斜 隸

次 州 存 趙 夙 凱 書

奇 夷 什 奮 門 古 方

字 前 二 蒙 競 碑 楷

葵

菌

雀

鳬

鵝

若

賴

米

荔

巢

雛

鵝

祆

覓

撼

蕊

檀

鳳

職

庶

纔

麥

苗

楊

隹

鬣

歷

屬

飧

鹵

李

蠶

龜

鴛

獻

蕭

鹽

杓

鼠

鼈

鸞

啟

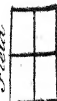
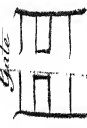


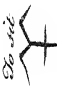



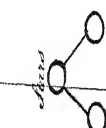






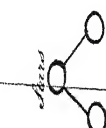



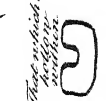
























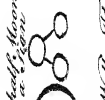



The vulgar character; called Tra'o Trée.

阮	书	阮	示	松	安	瑞
公	翺	光	示	松	安	瑞
姁	翺	光	示	松	安	瑞
桓	翺	光	示	松	安	瑞
雲	翺	光	示	松	安	瑞
持	翺	光	示	松	安	瑞

Kou ouen. *This is the most ancient Writing.* *Philo. Trunc. TOL. IX. TAB. XXV.*

<i>Don't disturb</i>		<i>Word speech</i>		<i>Connection</i>		<i>Middle</i>		<i>To digest stomach</i>		<i>Rich abundance</i>	
<i>Water</i>		<i>That which is above</i>		<i>Subject to a distance</i>		<i>Food</i>		<i>To fear</i>		<i>Mallet</i>	
<i>Rain</i>		<i>Right of the directions</i>		<i>Subject to a distance</i>		<i>Justice</i>		<i>Mythology</i>		<i>Bird that flies</i>	
<i>Rain</i>		<i>Western</i>		<i>Shiny diamond</i>		<i>Leaf</i>		<i>Burying place</i>		<i>On or On</i>	
<i>That which is without nature, which is not even</i>		<i>Brightly</i>		<i>Gate</i>		<i>Bird</i>		<i>Bow</i>		<i>Flag or pig</i>	
<i>Tiger or</i>		<i>Sail</i>		<i>Deep waters</i>		<i>Bough, or B. branch</i>		<i>Dragon or mythic</i>		<i>That of a bird</i>	
<i>Light famed</i>		<i>Dark, hollow</i>		<i>That which is in mouth</i>		<i>Dragon</i>		<i>Howe</i>		<i>lion</i>	

This letter is a true likeness of the Chinese carrying places round are trees; is in the middle, an Oval Solid or B. on which is written the Chinese of Earth which is over the Coffin. It is regarded as the figure of a Dragon or serpent, should have in it the two figures A. & N. which are the abstract figures of Man, very low the n or serpent in legend.

* This is still the form of the opening of the Chinese Wall. The word is very small: that the Moon may not therefore be mixed in, from the shape.

** This character is composed of 十 ten, crops, and 目 eye, which signifies Eyes, Death.

Ancient Character
of Fish

Ancient Character
of Fish

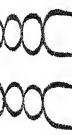
Id.



Id.



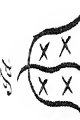
Id.



Id.



Id.



Id. (Note: to represent, as fish)



Id.



Id.



Id.



Id.



Id.



Id. (Note: in the black)



Id.



Id.



Id.



Id.



Id.



Id.



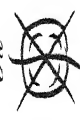
Present Character
of Fish

Id.

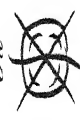


Present Character
of Fish

Id.



Id. (Note: as fish)



ancient character of
ground children
descending

Id. Id. Id. Id. Id. Id. Id.

Id. Id. Id. Id. Id. Id. Id.

Id. Id. Id. Id. Id. Id. Id.

Id. Id. Id. Id. Id. Id. Id.

Id. Id. Id. Id. Id. Id. Id.

Id. Id. Id. Id. Id. Id. Id.

Id. Id. Id. Id. Id. Id. Id.



ancient character
of Character

ancient character
of Character

Chinese characters approaching to some Hieroglyphics of Egypt

Chinese. Egyptian-Hieroglyphics

Meaning.  



Meaning.  



Meaning.  

Meaning.  

Meaning.  


Meaning.  

Meaning.  

Meaning.  

Meaning.  



Meaning.  

Meaning.  

Meaning.  



* This is spoken of the Earth, with respect to the Heavens.



Chinese. Egyptian-Hieroglyphics

Meaning.  

Meaning.  

Meaning.  



Meaning.  

Meaning.  



Meaning.  

Meaning.  

Meaning.  

Meaning.  

Meaning.  

Meaning.  


Meaning.  

* This is spoken of the Earth, with respect to the Heavens.

Chinese. Egyptian-Hieroglyphics

Meaning.  

Meaning.  



Meaning.  

Meaning.  


Meaning.  

Meaning.  


Meaning.  

Meaning.  

Meaning.  

Meaning.  

Meaning.  

Meaning.  

* This is spoken of the Earth, with respect to the Heavens.

No. 100 collection.

Chinese Char.^s also Egyptian Hieroglyphics

方

Square



卜

Divination



圓

Round



品

Abstemiousness



口

Mouth



五

Five



合

Union



夕

Evening



且

Immeasurable



合

Union



Phoen. Brev. No. LXX. LXXI. LXXII.

Chinese Char.^s also Egyptian Hieroglyphics

月

Shine



月

Moon



月

Moon



基

Conspicuousness



工

Work



淵

Deep, profound



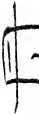
節

Rest, or Grace



丹

Dark colour



杵

Beat of a mortar



草


Grass



Constitution.

Chinese Char. in Egyptian Hieroglyphics

工  Chief, that which is above

上  Perfect First

下  Great, Lower opening

土  Heaven that which is above

王  King, Master


干  Opposite, contrary

個  Individual character

月  Moon

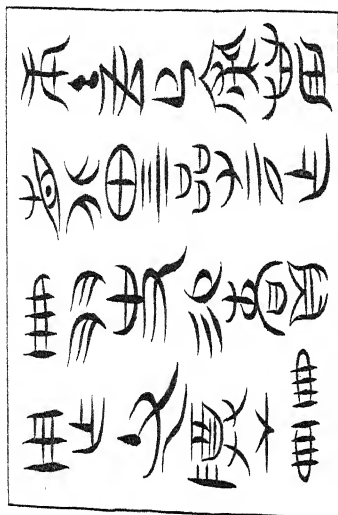
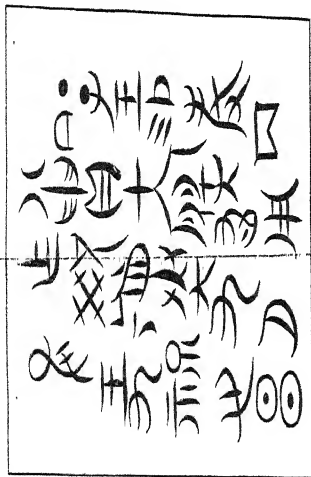
木  Tree

甲  Bullhide, This is so high

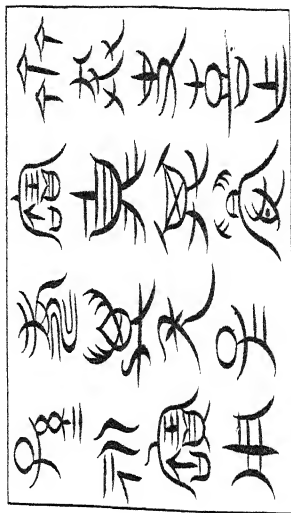
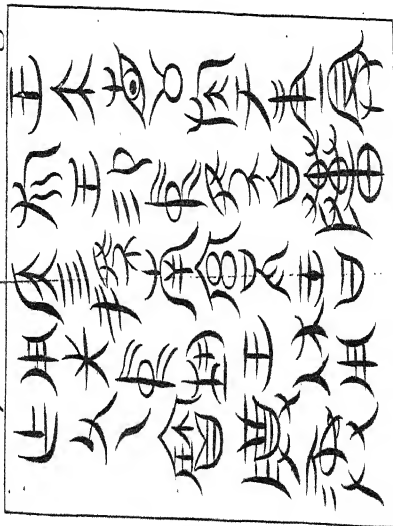
囚  Prisoner

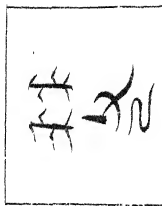
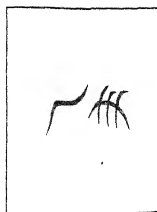
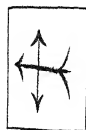
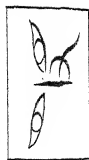
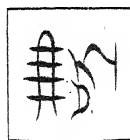
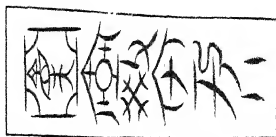
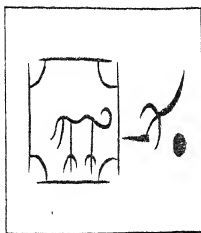
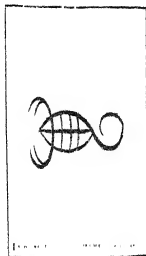
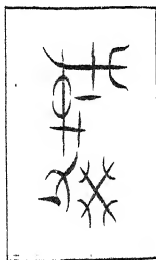
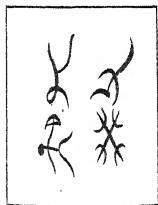
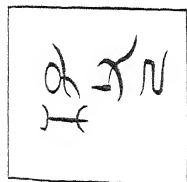
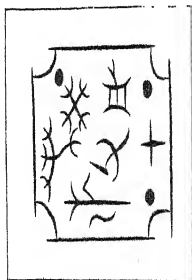
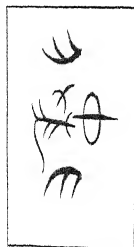
大  Large, Great, greater

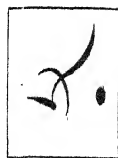
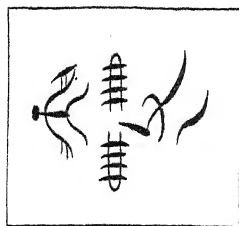
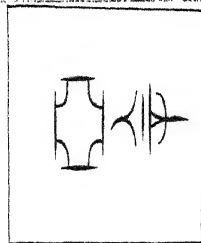
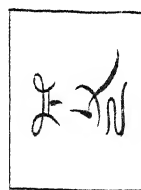
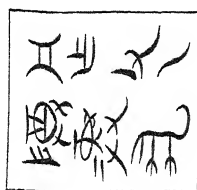
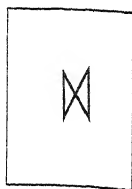
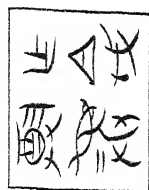
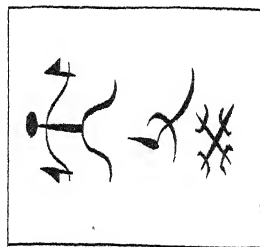
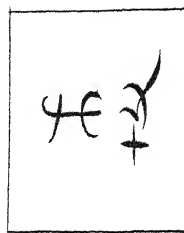
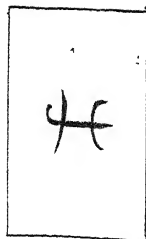
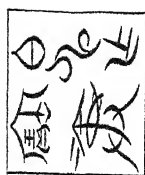
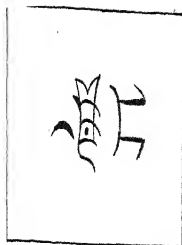
Inscriptions which are thought to be of the Dynasty of the Chang:



Inscriptions which are thought to be of the Dynasty of the Likou.







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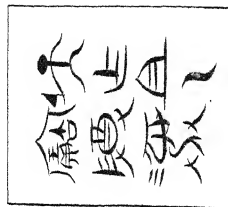
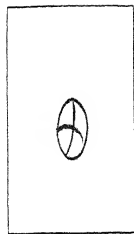
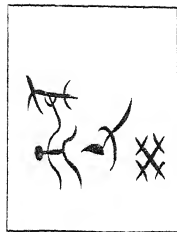
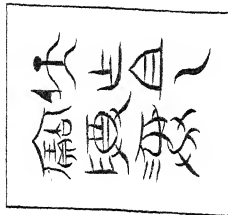
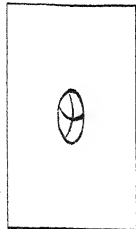
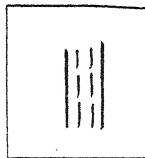
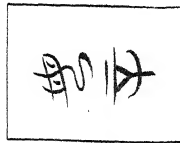
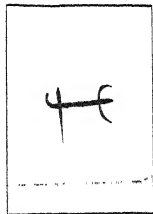
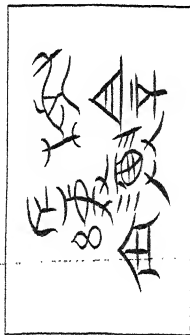
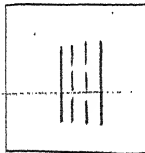
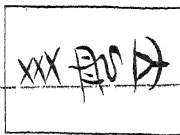
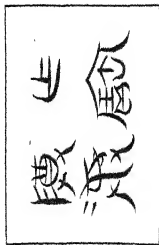
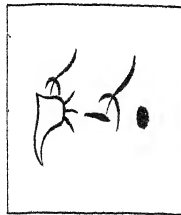
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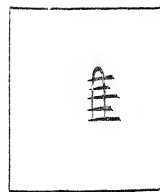
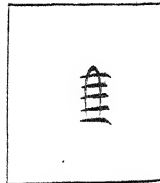
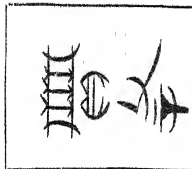
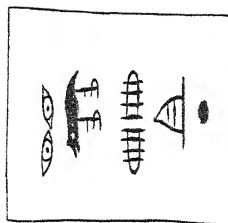
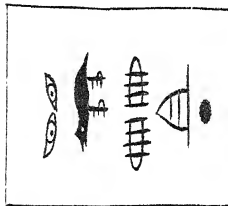
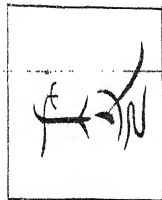
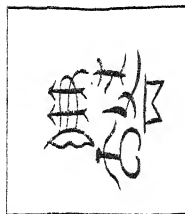
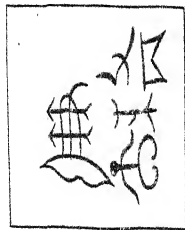
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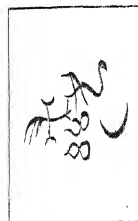
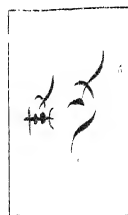
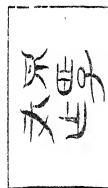
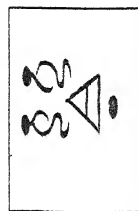
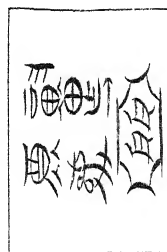
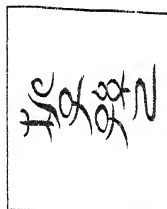
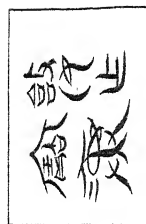
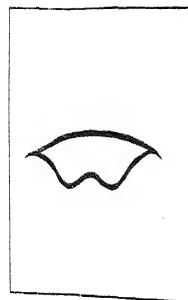
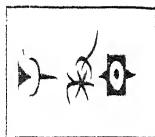
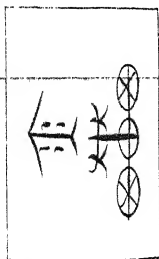
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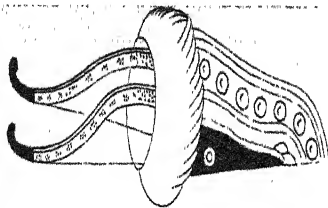
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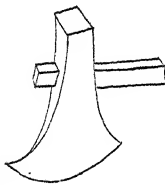
Chinese Antiquities.

Plates I. to LXX. TAB. XL.

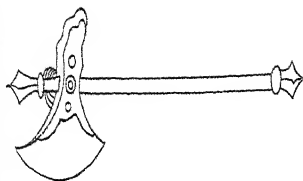
Bow & Case



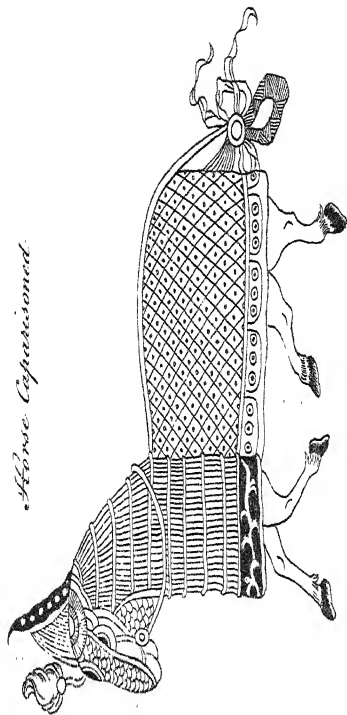
Carpenters' Axe



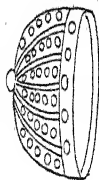
Axe used in War



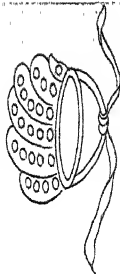
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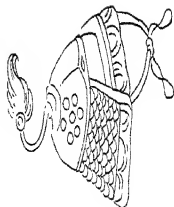
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Bonnet



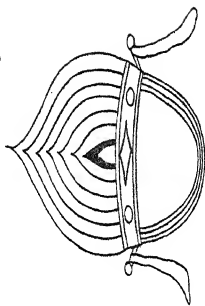
Casquette



Statue of a Lamb



Bonnet of Sacrificers

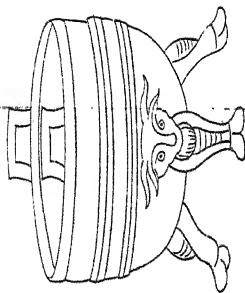


Bonnet of Ceremony

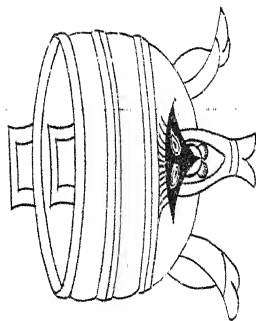


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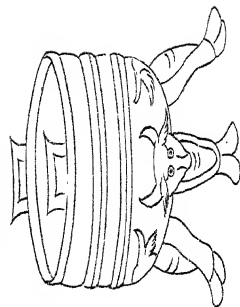
*Vessel for the Hall
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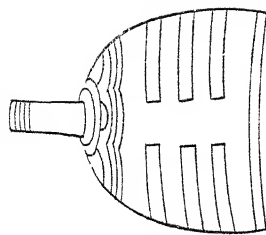
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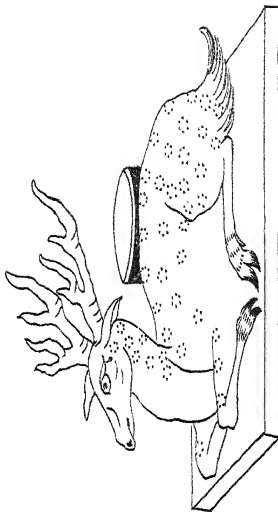
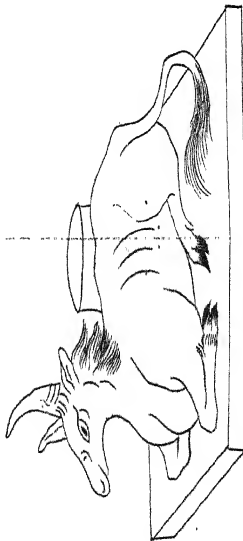
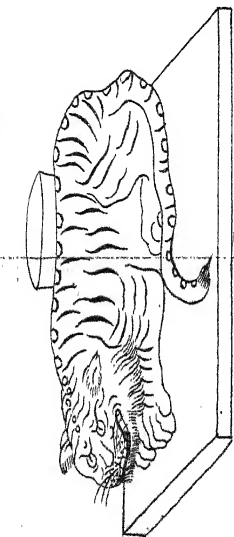
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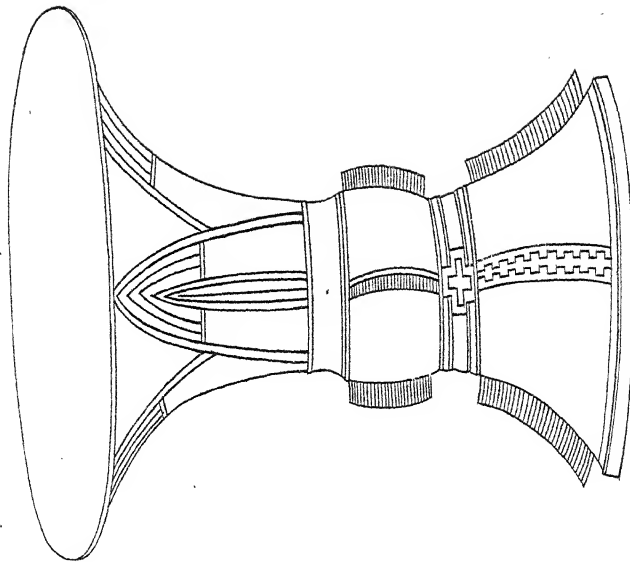
Bell



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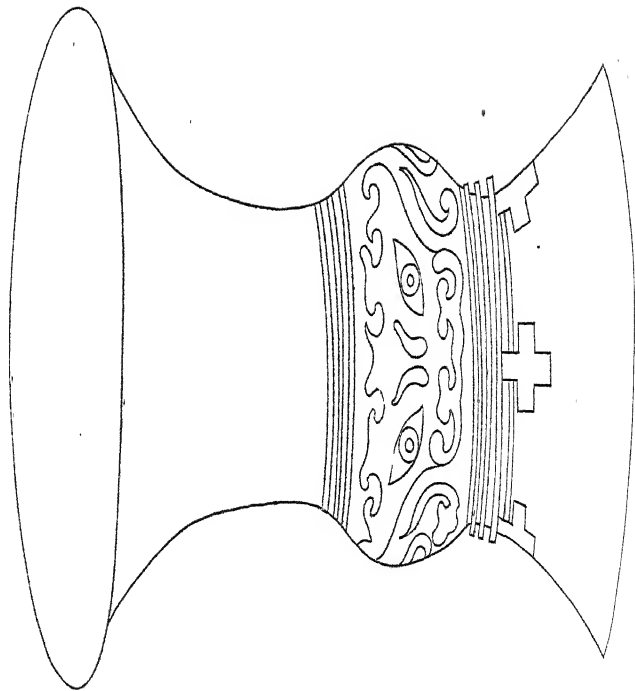


A Vase which is thought to be of the Dynasty of the Chang. The height is 1 Foot, 3 Lines; the Spout 9 Inches, 2 Lines; the depth 7 Inches, 1 Line; French Measure. Its Weight about 2 Pounds.

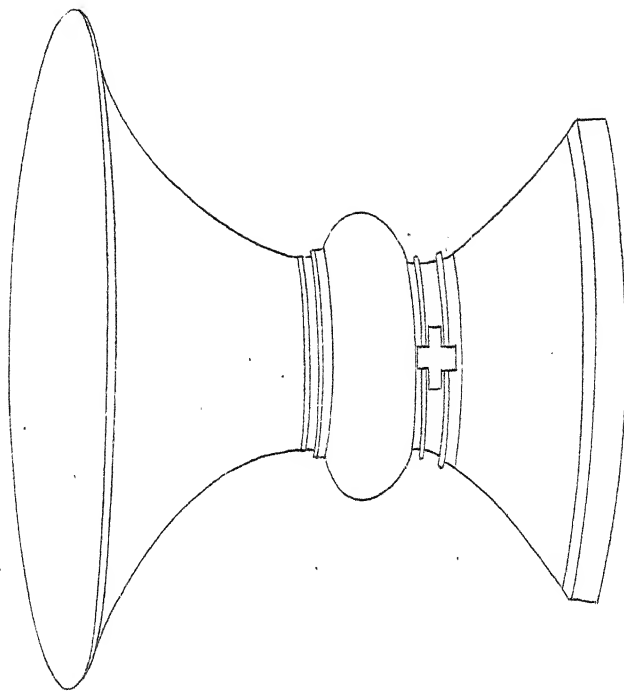


This Vase, & the two following, are remarkable for the Crochets which one sees clearly traced therein. I have found several others of this form, with Crochets; of which the Chinese say nothing. As certainly, as these Vases were for Sacrifices, & as they are the only ones that have Crochets: It is not credible, that this should be pure chance. However, since they have no inscription upon them; I would not warrant their being so Ancient, as the Chinese say they are. Perhaps they mount no higher than to the Hsien; or even the Tang.

A Vase which is thought to be of the Dynasty of the Sakers. The height is of 3
Inches, 5 Lines; with an aperture of 4 Inches, 3 Lines; & the depth of 4 Inches, 1
Line. It weighs a little more than 12 Ounces.



Another Case of the same Dynasty; its height 6 Inches, 5 Lines; the Aperture 4 Inches, 2 Lines; the depth 1 Inch, 3 Lines; Weight 13 Ounces.



LXVII. *Observation of the Transit of Mercury over the Sun, October 25, 1743. In a Letter to Professor Bliss, late Astronomer Royal and F. R. S. from John Winthrop, F. R. S. Professor of Astronomy in Cambridge, New England.*

REV. SIR,

Read at R. S. Nov. 10, 1763. **G**IVE me leave to lay before you an observation of the transit of Mercury, 25 October, 1743; which, if I mistake not, will determine the longitude of Cambridge, New England, with more exactness than any of the observations that have been used for that purpose. I adjusted my clock by correspondent altitudes of the bright star of Aries, taken the night before the transit with a quadrant of two feet radius; and on the day of the transit, by correspondent altitudes of the Sun; all of which agreed within 5''; and allowed for the difference of the Sun's declination, morning and afternoon.

The morning was fair and calm, but hazy; notwithstanding which, I had a good view of the Planet, and with a 24 feet telescope observed that

At 8^h 17' 5'' \propto in his egress touched \odot 's limb.
18' 58'' went off intirely.

VOL. LIX.

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I could not be so certain of the moment when he left the Sun, as of his interior contact. For the Sun's limb, undulating in the vapours of the horizon, made it somewhat difficult to judge when the indenture, formed by the Planet upon it, intirely ceased. However, I believe this latter observation may be relied on to 4 or 5". The comparison of this observation with those made in Europe will, I presume, determine the difference of meridians within a few seconds of time.

I beg, Sir, you will excuse the freedom I have now taken, since it is in the cause of astronomy, and allow me to subscribe myself, with great respect,

Reverend Sir,

Your most obedient,

humble servant,

Cambridge, New England,
20 June, 1763.

John Winthrop.

LXVIII. *A Method of working the Object
Glasses of Refracting Telescopes truly spher-
ical. By the late Mr. James Short,
E. R. S.**

Read Jan. 25, 1770. **P**REPARE two plates or tools of brass, the one convex, and the other concave, being both portions of a sphere of the same radius as the focal length of the object glass you want to have, or rather of a radius somewhat longer than the focal length you want, for a dioptrical reason; let these plates or tools be between two and three times the breadth of the object glass desired; or, in long focal lengths, twice the breadth will be sufficient: let these tools be of a sufficient thickness in proportion to their breadth or diameter, and let them be ground with fine emery exactly true to one another, working them alternately, the one above the other, to preserve the same focal length; or, if it is desired longer, you must work the convex above the concave; or, if desired shorter, you must grind the concave above the convex.

After this, you prepare another brass-plate or tool, of the same breadth and thickness as the two former,

* This paper, which was delivered, sealed up, by Mr. Short, at the Society, on the 30th of April, 1752, was, after his death, opened by the Council, and ordered to be printed.

and of the same radius of concavity ; its being truly turned on a lathe will be sufficient for this purpose ; which tool is to serve afterwards for the polishing of the two surfaces of your object glass, and therefore called the polishing tool.

Prepare a piece of straw-coloured glass, of the plate glass kind, of the proper diameter for the object glass you desire, which ought always to be broader than the proper aperture for that length ; let this piece of glass be ground flat, in another tool, on both sides, and as nearly parallel as may be, and somewhat polished, in order to discover whether there are any veins or flaws in the glass. When you are satisfied of the goodness of the glass, you are then to prepare a handle to fasten your glass to. Great care must be taken in this, for fear of bending your glass by the handle ; my method is this ; I take a flat piece of brass, or rather of the concavity of the sphere, to which the glass is to be ground ; this piece of brass should not be thicker than $\frac{1}{4}$ of the thickness of the glass, of a circular form, less in breadth somewhat than the glass itself, and having sides of the same form, at right angles to the flat piece of brass, and these sides ought to be of such a shape as that the fingers may easily apply to it in working, and these sides should be as low as may conveniently be, and no thicker than about $\frac{1}{8}$ of the glass. This handle is to be fastened to the glass, by warming the glass and handle gently before a fire, and laying some pitch upon the glass thus warmed, till it becomes soft like melted wax ; and then laying your brass handle, a little heated, on the pitch, you press it a little, till you are sure there is nothing between the
glass

glass and handle but pitch ; you then lay down the glass and handle upon something flat, taking care that the handle is in the middle of the glass, till it is entirely cold. It is very material to know, that the pitch, to be used for fastening the handle to the glass, must be soft pitch, that has never been used, nor melted ; for any other pitch will infallibly bend the glass.

You then grind your glass in the concave tool with emery, and give it the proper figure and smoothing for the last polish, in the common manner.

In order to give your glass the last polish, which is the most difficult part of the whole work, you are to prepare some pitch for covering the before mentioned polishing concave tool, which is done in this manner : Take some pitch, and melt it in an iron ladle, and let it boil for a quarter of an hour or thereabouts ; by this boiling, the pitch, when cold, will become hard and brittle ; or you may shorten this operation, by melting equal quantities of pitch and rosin, and then there is no occasion to let it boil so long. Your pitch being thus prepared, you again melt it, and take it off the fire, and let it stand till the pitch becomes pretty cold, or of a thickish consistence ; and having warmed the polishing tool a little, to make the pitch stick to it, you pour out of the ladle upon the polishing tool as much of the pitch as you judge will cover the whole tool, when spread out, to about the thickness of $\frac{1}{4}$ of an inch ; you then invert this tool with the pitch upon it, and press it upon the convex tool, which must be quite dry, clean, and cold, in order to give it the figure of the convex tool ; in case it has not spread out so as to cover the whole surface
of

of the polishing tool, you warm the pitch by holding it before the fire, and pressing it upon the convex tool, as before, till it has entirely covered the surface of the polishing tool ; you then plunge it into cold water, till the brass is quite cold.

N.B. In order to know if your pitch is hard enough, you press the edge of the nail of your thumb upon it, and if it receives an impression, the pitch is not hard enough.

You then proceed to prepare this polishing tool, for the last polish of your glass, by grinding this polishing tool upon the convex tool with pretty coarse emery, and a small quantity of water, in the common way that tools are ground one upon another ; but this must be done only for a small space of time, and the polishing tool must have no other pressure than its own weight, for fear of some of the emery sticking in the pitch, and you must never allow the emery to grow dry ; when you have ground the pitch so as to be all over of the same colour, you then wash the pitch from all the emery with a brush and clean water ; after this you take a bottle of water, and holding the pitch tool in a sloping position, you pour water out of the bottle so as to fall upon every part of its surface.

You then place the polishing tool in a horizontal position, and you put upon it some putty, washed from all its gritty particles, but it need not be the finest washing, and you put a good deal of water upon your polishing tool, mixing the putty and it together, and you polish your glass upon this pitch polisher in the common manner of polishing glasses.

After

After you have polished your glass about ten minutes, you again grind your polisher upon the convex tool with emery, as before, for fear the pitch has, by working, lost any of its proper figure; and the oftener you do this, the truer will be the figure of your glass; and in this manner you proceed till the glass is quite polished.

You then take your glass off its handle, by holding it before the fire, till it is so warm that you can slide the handle off the glass; and whilst the glass is warm, you take off as much of the pitch as you can with the sharp edge of a knife; you then lay the glass down to cool, and, when quite cold, you drop some spirits of wine upon it; and this, with a cloth, will wipe off the rest of the pitch.

You then examine the center of the surfaces of your glass; and if it lies to one side of the center of your glass, mark that place with a spot of ink, and then put on your handle as before, upon the side that is now polished, with its center over the spot of ink, and grind your glass as before, till the circular remaining part of the glass to be ground is as much distant from the center of the glass on the other side from the spot as the spot was from the center of your glass; you then by heat return your handle to the center of the glass, and proceed to grind and polish this side of the glass as before.

N. B. The concave and convex tools should be ground with fine emery, after you have done one side of your glass; for the oftener these are ground together, you will be the more sure of having your figure true.

ERRATA.

- P. 153, l. 23, *for* knew *read* know.
P. 154, l. 10, *for* 9'' -- 3'', 394 *read* 9'' -- 3'', 394.
P. 187, l. 24, *for* that the *read* that though the.
l. 27, *for* with respect to obstructing *read* with respect
to him in obstructing.
P. 434, l. 21, *for* EUGUBINI *read* IGVINI.

INDEX

A N
I N D E X
T O T H E
Fifty-Ninth V O L U M E
O F T H E
Philosophical Transactions.

For the YEAR 1769.

A.

ABELLE, or white poplar, doubts about its being an indigenous English tree, p. 37.

Achromatic telescopes, required for some nice astronomical observations, p. 459.

Acids, not the principal cause of the suspension of iron in medicated waters, p. 222, &c.

Air, its expulsion is probably the cause of the lateral force of electrical explosions, p. 60, 61. Fixed, serves to suspend calcareous earths in water, p. 217. And likewise iron, p. 218. &c. Has a greater affinity with alkalies than with iron, p. 223.

VOL. LIX.

U u u

Alemoor

- Aleuicor* Lord, his observation of the transit of Venus, p. 339, 340. Has built an observatory at Hawk-hill, near Edinburgh, p. 343.
- Alkalies*, their effects on chalybeate waters accounted for, p. 223, &c.
- America*, its animals less in size than in the other parts of the globe, p. 127.
- America*, North, increase of its inhabitants, p. 121. In 70 years the number will be double of those in Great Britain, p. 122.
- Animalcula* produced from infusion, infinitely smaller than the spermiatic animals, p. 191. Some of them produced from the infusion of hemp-seed, p. 142. Their division owing to some accidents, p. 143. Swim by means of minute fins, p. 144.
- Animals*, many species formerly known and now extinct, p. 135.
- Arachis*, a plant, from whose pods a good oil is extracted, p. 330.
- Archimedes's* Mirrors imitated at Dresden, p. 8.
- Astronomer* Royal, his remarks on the observations made at Isle Coudre, near Quebec, p. 280. On those of Hawk-hill, near Edinburgh, p. 343, 366. On those at Gibraltar, p. 350. His communication of the eclipses of Jupiter's first satellite at Greenwich, p. 399. Remarks on the observations of the transit of Venus at Leweston in Pennsylvania, p. 420.
- Atmosphere* of Venus suspected by Mr. Hirst, p. 231. By Dr. Smith of Philadelphia, p. 316.
- Aubert*, Mr. Alexander, his observation of the transit of Venus, three seconds E. of St. Paul's, p. 378.
- Aurora boreales*, two observed at Paris, p. 86. Two observed at Oxford, p. 367.

B.

Barometer, very low, p. 85, 156, 159.

Barriington

- Barrington*, Honourable Daines, on trees supposed indigenous in Great Britain, p. 23, &c.
- Bayley*, Mr. William, his astronomical observations at the North Cape, p. 262. Could observe but imperfectly, on account of the clouds, the transit of Venus, p. 266. Saw a black ligament between the limbs, *ibid*.
- Bayly*, Mr. Joel. See *Biddle*.
- Beasts*, many species destroyed in different countries, p. 135. Some, perhaps, intirely extinguished, *ibid*.
- Beroe*, a kind of gelatinous marine animal, p. 144. Its similarity with the *Volvox*, an animal produced by infusion, *ibid*.
- Bevis*, Dr. John, his observation of the transit of Venus, p. 189. Saw a slender tail between the planet and the sun, p. 190. Observes the eclipse of the sun, p. 191.
- Biddle*, Mr. Owen, and Mr. Joel Bayley, appointed by the Philosophical Society at Philadelphia to observe the transit of Venus at Lewestown in Pennsylvania, p. 414. Their observations, p. 416.
- Bills* of mortality very defective in London, p. 120. How they might be improved, p. 124. Defect of those at Paris, p. 120.
- Births*, more deficient than the burials in the London bills of mortality, p. 100. In what proportion probably, p. 101. That proportion lessened, p. 108, 109.
- Bodies*, drawn constantly towards two fixed points, describe equal solids in equal times, p. 76.
- Bones* fossil, found in North America, p. 126; and in Siberia, p. 127. Whether carried thither from other parts, p. 130, 131. Of a species probably indigenous, 133. Differing from the elephants, p. 132. Now extinct, p. 135. Probable cause of this phænomenon, p. 136, 137.
- Bones* regenerated, p. 41, 42. In what manner probably, p. 43.
- Bonn*, figures and inscriptions found in that place, p. 195.
- Borlase*, Dr. William, communicates an information about native tin found in Cornwall, p. 47, 48. His meteorological observations in Cornwall, p. 157.

- Boston* in New England would decrease in inhabitants, were there no supply from the country, p. 115.
- Box*, not an indigenous tree, p. 29, 35.
- Brakenridge*, Dr. his method of finding the number of the inhabitants in London precarious, p. 102, 103. Has over-rated it, *ibid.* An error in another method of his, p. 104.
- Brownrigg*, Mr. George, sends a particular oil extracted from ground nucs in North Caroline, p. 379.
- Burials* exceed births in towns, p. 115. Less in Paris than would be expected, p. 119.

C.

- Canton*, Mr. John, observes the transit of Venus, p. 193, 194. And the eclipse of the sun, p. 194. His experiments on the luminousness of the sea, 446.
- Cayaneburg* in Lapland, observations of the transit of Venus in that place, p. 329.
- Cartilages* regenerated as well as bones, p. 44.
- Chalybeate* waters, imitated by Mr. Lane, p. 218—222, &c.
- Chelsea*, fifty new plants of the garden described, p. 384, &c.
- Chestnut*, doubts about its being originally a native of Great Britain, p. 24, 25, &c. Whether formerly much employed as timber, p. 26. The oldest of these trees in England, p. 30.
- Chinese*, a great nation from the time of the confusion of tongues, p. 493. Their early and uninterrupted ideas of a Deity, p. 503, 504.
- Chinese* letters, anciently symbolical, p. 495. Afterwards hieroglyphical, p. 497. How formed, *ibid.* Their differences, p. 498, 501. Defects and irregularities, p. 499, 500. Their resemblance with Egyptian hieroglyphics, p. 502; but intirely different, *ibid.*
- Chyle*, transparent in birds, p. 199. White in the crocodile, *ibid.*

- Climates*, a change of, improbable, p. 131.
Cobwebs, why not found on high and dark roofs, p. 30,
 31. More common on white-wash than on wainscot,
 ibid.
Conserva, a particular species found near Cortona, in the
 form of a kind of paper, p. 50. Different species of
 that plant, p. 53. Errors of botanists about them,
 ibid. Artificial paper made from the same substance,
 p. 53, 54. What the *Conserva Plinii* was, p. 52—56.
Cuticle, the whole separated from the hands and feet of a
 patient in a fever, p. 282. A glove detached from
 the hand, p. 283.

D.

- Dean*, no chefnuts to be found in the forest there at pre-
 sent, p. 29.
Dena does not necessarily imply the forest of Dean, p. 28.
Denarius Etruscan, explained by Mr. Swinton, p. 440.
Dixon, Mr. Jeremiah, his astronomical observations on the
 island of Hammerfest, near the North Cape, p. 253.
Dresden, its foot's proportion to that of London, p. 5.
 Burning speculums of a superior force executed there,
 ibid.
Dublin, probable number of its inhabitants, p. 105.
Ducarrel, Dr. his opinion about chefnut-trees examined,
 p. 27, 28, 29.
Dunn, Mr. appearances observed by him during the transit
 of Venus, p. 232, 356, 357.
Dymond, Mr. See *Wales*.

E.

- Earthquakes* at Macao, p. 71. Very violent at Manilla,
 p. 72.
Eclipses of Jupiter's first fatellite observed at Greenwich,
 p. 401. At Glasgow, p. 402.

Eclipse

- Eclipse* of the moon in 1769, observed at Edinburgh, p. 369. At Greenwich, p. 366, 399.
- Eclipse* of the sun in 1769, observed at Shirborn-castle, p. 174. At Oxford, p. 178. At Kew, p. 191. In London, p. 194. At Leicester, p. 239. At the North Cape, p. 259—268. Near Leeds, p. 286. At Cajaneburgh, 330. Near Edinburgh, p. 345. At Gibraltar, p. 349. At Paris, p. 375. At Stockholm, p. 406. At East Dereham in Norfolk, p. 409.
- Egyptian* letters, whether the same with the ancient Chinese, p. 490, 491. Not at all known at present in China, p. 502.
- Electrical* explosions, their lateral force, p. 57. Experiments to ascertain its cause, p. 58, &c. Probably produced by the expulsion of air, p. 60. What sensation it produces, p. 62.
- Electrical* machine, observed at Vienna to acquire an uncommon force during an aurora borealis, p. 88.
- Electricity*, its direct passage through conductors may be traced, p. 63. Its effects, *ibid.* Its force not affected by the inflexions of the wires, p. 64. But much lessened in proportion to their length, p. 65, 66, 67. A very short passage through the air, taken in preference to a long one through metals, p. 68, 69. May have some share in the fall of rain, p. 361.
- Elephants*, true, probably never were in America, p. 139. Differing in diverse parts, p. 136.
- Ellis*, John, Esq; on the animalcula of vegetable infusions, p. 138, 139, &c. Discovers an indissoluble salt in an infusion of hemp-seed, p. 145, 146. And also in infusions of flax and of wheat, p. 198.
- Elm*, probably not an indigenous tree of England, p. 34.
- Ericaulon decangulare*, a rare plant found in the isle of Skie, p. 293.
- Ever-green* trees, none indigenous in England, but holly, juniper, and ivy, p. 36.

Expectations of life, new calculated from the bills of mortality, p. 89. Two senses affixed to that expression, p. 90. Double for a child in Madeira to one in London, p. 113.

F.

Families, may, upon an average, consist of five persons in London, and of less throughout England, p. 113.
Farr, Dr. William, his meteorological observations at Plymouth, p. 81.
Fir, Scotch, not found in any part of England, p. 32. Subterraneous, dug out in many places, *ibid*. Was probably formerly an indigenous tree in the northern parts of England, p. 33, 34.
Fou-ki introduced the eight elementary characters in the Chinese writing, p. 495.
Fungi, their quick vegetation on moist animal and vegetable infusions, p. 139.

G.

Giants bones found in several places, p. 127. Belonging to some large animals, p. 128.
Gleffes, how to be worked truly spherical, p. 507.
Gibbon, an ape without tail of the East-Indies, p. 73. Length of his arms, *ibid*.
Glands, none of the lymphatic kind in turtles and in fish, p. 210.
Gooch, Mr. B. his account of a singular separation of the cuticle in a disorder, p. 281. Sends that of the hand of the patient, which has the form of a glove, p. 283.

H.

Haddock, its lymphatics described, p. 205.
Harris, Mr. Daniel, his observations of the transit of Venus at Windsor, p. 422.

Hamilton,

- Hamilton*, Honourable William, his letter on Mount Vesuvius, p. 18. His account of some other volcanoes in the neighbourhood, p. 19, 20.
- I'berden*, Dr. Thomas, his account of the populousness at Madeira, p. 111.
- Heberden*, Dr. William, on the different quantities of rain at different heights, p. 359. His suspicions on the cause of this phenomenon, p. 361.
- Hemp-seed*, an infusion of it shews *animalcula* multiplying by accidental division, p. 142. Produces an indissoluble salt, p. 145, 146.
- Hewson*, Mr. John, on the lymphatic system in amphibious animals, p. 198. On the lymphatics in fish, p. 204.
- Hirst*, Rev. Mr. William, observes the last transit of Venus at Greenwich, p. 228, 229. Had observed the preceding one at Madras, p. 229. Saw in both a protuberance from the planet at the time of the internal contact, p. 230. And a penumbra preceding the external contact, p. 231, &c.
- Holland*, Samuel, Esq; his astronomical observations in North America, p. 247. Observes the transit of Venus near Quebec, p. 249.
- Hope*, Professor John, his account of a rare plant found in the isle of Skie, p. 241.
- Hornsky*, Professor Thomas, his observation of the transit of Venus at Oxford, p. 174, &c. Sees the limbs of the planet and the sun united at the internal contact by a kind of ligament, p. 176. Observes the eclipse of the sun, p. 178, 179.
- Horsfall*, Mr. James, his observation of the last transit of Venus, p. 170.
- Horsley*, Rev. Mr. on the computation of the sun's distance from the earth, p. 153. Observes the transit of Venus at Oxford, p. 183, &c. Sees a kind of ligament between the planet and the sun at the internal contact, p. 184.
- Hudson*, Mr. William, his account of the fifty Chelsea plants for 1768, p. 384.

Hunter,

Hunter, Mr. John, discovered the lacteals in a crocodile
p. 204.

I.

Yardine, Lieutenant, his observation of the transit of Venus at Gibraltar, p. 347. And of the eclipse of the sun, p. 319.

Jesuits at Peking, their answer on the supposed affinity between the Chinese and Egyptian letters, p. 489.

Inhabitants. See *Number*.

Inscriptions found at Bonn, p. 196.

Jones, the late William, Esq; his demonstration of the law of motion in bodies, constantly drawn towards two fixed points, p. 75.

Iron, by becoming rusty, ceases to be a conductor of electricity, p. 168. Suspended in water by means of fixed air, p. 218. Combined with acids in the pyrites, p. 217.

Jupiter, eclipses of his first satellite observed, p. 401, 402, 454, 461. Shadows of the satellites, p. 457. Variations of his belts, p. 457.

K.

Knots upon cords used before letters in China, p. 495. Analogy between this and what was observed in America, *ibid*.

L.

Lacteals in turtles, p. 199. In fish, p. 205. Form a network of vessels between the muscular and villous coats of the intestines, p. 210.

Lalande, Mr. De, sends the observations of the transit of Venus made at Paris, p. 374.

- Lane*, Mr. Tim. his experiments on the solubility of iron in water, by the intermission of fixed air, p. 216, &c.
- Law* of motion. See *Bodies*.
- Leeds*, Mr. John, his observation of the transit of Venus in Maryland, p. 444.
- Leicester*, its latitude, p. 290.
- Licou*, the different characters of the Chinese, p. 495, 496.
- Ligament* between the limbs of Venus and the Sun at their internal contact, seen by several observers, p. 176, 184, 185, 231, 266, 276, 331, 417.
- Light*, a border of, seen round that part of Venus which was yet off the Sun, p. 310, 312.
- Lime* tree imported into England, p. 35.
- Lind*, Doctor James, his observation of the transit of Venus, at Hawkhill, near Edinburgh, p. 339, &c. Of the eclipse of the Sun, p. 342. And of an eclipse of the Moon, p. 363.
- London*, the number of its inhabitants, p. 99. 102. was decreasing for thirty years, p. 107. Less fatal to children than formerly, p. 109. Fewer people attain to very great ages than in other places, p. 116. Is now increasing, p. 118.
- Ludlam*, Rev. Mr. observes the transit of Venus at Leicester, p. 236. And the Sun's eclipse, p. 239.
- Luminousness* of the sea owing to the putrefaction of animal substances, p. 446. May be imitated in artificial sea-water, p. 448. Destroyed by a certain heat, p. 449.
- Observed and accounted for by several navigators, p. 450.
- Lymphatic* System discovered in amphibious animals by Mr. Hewson, p. 198. And in fish, p. 204. Have no valves in these last, p. 210. How to be found in them, p. 211.
- Lymphatics* of the urethra and bladder, how discovered, p. 395. Described, p. 396, 397.

Joyson, Dr. of Gloucester, letter to Dr. Nicholls on a case of three pins swallowed by a girl, and discharged at her shoulder, p. 19.

M.

Macclesfield, the Earl and Countess of, observe the transit of Venus at Shirburn-castle, p. 173, 174.

Machin, Mr. indicates a law of motion in bodies, deflected by two forces directed to two fixed points, p. 75, 76.

Madeira, life of the inhabitants more settled than in London, p. 112. Doubles its inhabitants in 84 years, p. 120.

Mammout, fabulous animals of Siberia, p. 127.

Maskelyne, Mr. communicates the eclipses of Jupiter's first satellite, the eclipse of the Moon, and the occultations of fixed stars by the Moon, observed at Greenwich in 1769, p. 399. See *Astronomer Royal*.

Mayer, Professor Andrew, his observation of the transit of Venus at Gryphiswalde, p. 284.

Mercury, transit of, over the Sun in 1743, observed in New England by Professor Winthrop, p. 505.

Messier, Mr. his account of two remarkable auroræ boreales at Paris, p. 86. Observes the transit of Venus at Paris, p. 376. Astronomical observations made and communicated by him, p. 454.

Metallic communication, from the crosses or weather-cocks of steeples down to the ground, necessary to preserve them from injury by lightning, p. 163.

Meteorological observations at Plymouth, p. 81. At Bridgwater, p. 155, 156. At Ludgvan in Cornwall, p. 157—159.

Mirrors, caustic, made at Dresden, p. 4. Their construction, *ibid.* Their dimensions, p. 5. Their effects, p. 6, 7, 8.

Moivre, Mr. Abraham De, his hypothesis on the equal decrements of human life, p. 90, 91.

Monkeys, singular species without tails, from Bengal, p. 72.

Probably the same with Buffon's Gibbon, p. 73.

Montagu, Edward Wortley, his controversy with Mr. Needham about the bust at Turin, p. 490.

Moon's transit over the Pleiades in 1767, observed at Paris, p. 460. Two eclipses in 1768 observed at Paris, p. 463, 465. One in 1769 observed at Edinburgh, p. 364. And at Greenwich, p. 366, 399.

Monro, Professor, saw the lacteals in a turtle, p. 204.

Morris, Mr. Corbyn, error in his computation of the inhabitants of London, p. 151.

Morton, Dr. Charles, his extract of a letter from the Jesuits at Pekin on the Chinese characters, p. 489.

Mouldiness, occasioned by particular fungi or mushrooms, p. 139. Not to be confounded with the animals that feed on them, p. 140.

Moult, Mr. John, on a new manner of preparing salep, p. 1.

Mountains are produced by volcanoes, p. 21.

N.

Needham, Mr. Turbervill, his observations on the an malcula found in several infusions, p. 138, 139. Corrected by those of Mr. Ellis, p. 140. His opinion on the similarity between the Egyptian and Chinese characters, p.

New England, increase of its inhabitants, p. 122. Their actual number, p. 123.

Nicholls, Dr. John, his letter, inclosing an uncommon case, p. 9.

North Cape, its longitude, p. 266. And latitude, p. 270. Meteorological observations made there, p. 270, 271.

Norway, number of very old people in that country, p. 117.

Number of inhabitants may be deduced from the bills of mortality of a place, p. 97. What it probably is at Breslaw, *ibid*.

In London, p. 98, 99, 102. At Rome, p. 105. In Berlin, *ibid.* At Dublin, *ibid.* At Norwich, p. 114. In the Prussian dominions, p. 115.

O.

Oak, often mistaken for chesnut in old buildings, p. 26.
Ohio, fossil bones found near that river, p. 126.
Oil, extracted from the pods of the *Arachis*, p. 385. Is not apt to grow rancid, p. 381. Mentioned by Sir Hans Sloane, *ibid.* Its low price, p. 382.
Orchis, roots of, may be prepared so as to resemble the salep, p. 1. What species is the properest, p. 2.

P.

Paper, natural, found near the city of Cortona, p. 50. Its origin, p. 51. Artificial made from the same substance, p. 53, 54.
Paris, some of its encouragements to marriage and population, p. 119. Its bills of mortality more compleat than those in London, p. 120. — Excepting with regard to the ages of the dead, *ibid.*
Paxton, the Rev. Mr. William, his letter on the effects of a thunder-storm on the tower and the church of Buckland Brewer, p. 79.
Pekin. See *Jesuits*.
Philadelphia, Philosophical Society of that city appoints several committees to observe at different places the transit of Venus, p. 290, 414, &c.
Pingré, Mr. observes the transit of Venus at Cape François, p. 376.
Pins, three swallowed by a girl, p. 10. Symptoms which attended that accident, p. 11. Discharged at her shoulder, p. 12. Conjectures on their passage thither, p. 13—16. Case similar to it, p. 17.

Plenman observes the external contact of the transit of Venus at Cajamburgh, p. 330. And the eclipse of the Sun, *ibid.*

Price, Mr. Richard, his observations on expectations of life, &c. p. 89, &c.

Priestly, Dr. on the lateral force of electrical explosions, p. 57, 58. On the force of electrical explosions, p. 63, &c.

Putrefaction of fish makes the sea-water luminous, p. 447. Salt necessary for this effect, *ibid.* How accounted for, p. 449.

R.

Rain, unequal quantities of it fall at top or at bottom of high buildings, p. 361.

Raspe, Mr. his dissertation on the fossil bones found in North America, p. 126.

Robertson, Mr. John, his letter to the President, p. 74. Communicates the demonstration of a law of motion in bodies deflected by two forces tending to two points, by the late Mr. Jones, p. 75.

Rosewarne, Mr. his account of a specimen of native tin found in Cornwall, p. 49.

Royal Society consulted about the means of securing the cathedral of St. Paul's from damage by lightening, p. 160. Report of a committee in consequence of this application, p. 162, &c. Sends observers to the North Cape on account of the transit of Venus, p. 253. And to Hudson's bay, p. 480.

S.

Safnim, the name of a Samnite family, p. 437.

Saint Paul's, the Dean and Chapter of, consult the Royal Society about the best means of securing their cathedral from damage by lightening, p. 160. How the building may be secured, p. 162, &c.

Salep,

- Salep*, that which comes from Turkey may be imitated by means of the roots of the Orchis, p. 1. In what manner, p. 2. Its power of thickening of water and making a jelly, p. 3.
- Salt* indissoluble produced from an infusion of hemp-seed, p. 146, 147. And from some other vegetable infusions, p. 148.
- Sammite* coins elucidated by Mr. Swinton, p. 432.
- Saturn*, two dark belts discovered on his globe, p. 459.
- Sea*. See *Luminousness*.
- Short* Dr. his observations on bills of mortality commended, p. 110.
- Short*, late Mr. James, his method of working spherical glasses, p. 507.
- Shoulder bone*, the head of it sawed off, and the motion and figure of the arm preserved, p. 40, 41. The same operation recommended, p. 44.
- Sicamore*, or Great Maple, a tree of foreign extraction, p. 35.
- Skie*, a rare aquatic plant found in that isle, p. 241.
- Smeaton* Mr. John, his observations of the solar eclipse, near Leeds, p. 286.
- Smith*, Dr. William, provost of the college of Philadelphia, his account of the transit of Venus observed by him and several other gentlemen, at Norriton, twenty miles N. W. of Philadelphia, p. 289, &c.
- Solfaterra*, an extinguished volcano, p. 20.
- Spherical* glasses, how to be worked, p. 507.
- Steward*, Dr. his method of determining the distance of the Sun, true in its principle, p. 153. Imperfect in its use, p. 154.
- Stockholm*, the observations of the transit of Venus in that city, p. 332.
- Strange*, John, Esq; his account of a natural paper found near Cortona, in Tuscany, p. 50, 51. Describes several sepulchral inscriptions and figures found at Bonn, p. 195.

Swinton Rev. Mr. John, his account of two Auroræ Boreales observed at Oxford, p. 367. His elucidation of two Samnite coins, p. 432.

T.

Thunder cloud, bursting on the tower of a church, threw some of the large stones at a great distance, p. 80.

Tin native, found in Cornwall, p. 49.

Transit. See *Venus*.

Transit telescope by Mr. Woolaston, 411.

Trees commonly supposed indigenous to Great Britain, p. 23. What characters such trees should have, p. 23. 24.

Turin, bust at, occasions some dispute, p. 490.

Turtles, their lymphatics traced, p. 199, &c.

V.

Venus, its transit over the Sun observed in London, p. 170.

192. At Shirburn Castle, p. 173. At Oxford, p. 174. 183. At Kew, p. 189. At Greenwich, p. 228. At Leicester, p. 236. At and near Quebec, p. 249. 273. Near the North Cape, p. 259. At Cajaneburg, p. 330. At Upsal, p. 331. At Stockholm, p. 332. At Glasgow, p. 333. At Hawkhill near Edinburgh, p. 339. At Kirknewton, p. 344. At Gibraltar, p. 347. At Cambridge in New England, p. 351. At Paris, p. 374. 376. At Bourdeaux, p. 375. At Brest, *ibid.* At Cape François (St. Domingo), p. 376. At Martinico, *ibid.* At East Dereham in Norfolk, p. 407. At Leweston in Pennsylvania, p. 414. In Windsor Castle, p. 422. In Maryland, p. 444. At Hudson's Bay, p. 480.

Vesuvius, Mount, formed by degrees, p. 19. its ashes pernicious to hogs, *ibid.*

Villi of the intestines, consist of a net-work of lacteals, and of arteries, and veins. p. 212.

Visme, Stephen De, Esq; his letter on an earthquake at Macao, and a singular species of Monkies in the interior parts of Bengal, p. 71.

Volcanoes, several in the neighbourhood of Mount Vesuvius, p. 19, 20, 21.

Upsal, observations of the transit of Venus in that city, p. 331. The ligament between the limbs of Venus and the Sun, seen there, *ibid*.

W.

Wales, Mr. William, and Mr. Joseph Dymond, their astronomical observations, by order of the Royal Society in Hudson's Bay, p. 467. Their observations on the transit of Venus, p. 480.

Wargentin, Mr. sends the accounts of several observations of the transit of Venus in Sweden, p. 327. His observation at Stockholm, p. 332.

Watson, Mr. Henry, his description of the lymphatics of the urethra and neck of the bladder, p. 392.

Watson Dr. William, his account of an oil sent over from North Carolina, p. 379, &c.

White, Mr. his account of a case where the *os humeri* was sawed off, and the motion of the limb preserved, p. 39.

Wilson, Dr. Alexander, observations of the transit of Venus at Glasgow, p. 333, &c. Saw the ligament between the limbs of Venus and the Sun, p. 335. His observations of eclipses of Jupiter's first satellite, p. 402.

Winthrop, John, Esq; his observations of the transit of Venus at Cambridge, in New England, p. 331, &c. Of the transit of Mercury, in the same town, in 1743, p. 505.

Wollaston, Rev. Francis, his observations of the transit of Venus at East Dereham, in Norfolk, p. 407. And of the

the eclipse of the Sun, p. 409. Account of his transit telescope, p. 411.

Wolfe, Dr. on some caustic mirrors constructed at Dresden, p. 4.

Wright, Mr. Thomas observes the transit of Venus at Isle Coudre near Quebec, p. 273. Saw the ligament between the limbs of the planet and the Sun, p. 276.

Writing, probably established in the antediluvian world, p. 493.

Y.

Yew, doubts about its being an indigenous tree in England, p. 36. Description of one of an extraordinary size in a Scotch church yard, p. 37.

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